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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Sarcoma Uteri.* By ALEXANDER RUSSELL SIMPSON, M.D., Professor of Medicine and Midwifery and the Diseases of Women and Children in the University of Edinburgh.

IT is one of the many services which Virchow has rendered to pathology, to have rescued the term Sarcoma from the somewhat vague senses in which it used to be employed, and to have applied it to a group of tumours whose source he has carefully traced, whose structure he has elaborately described, and whose relation to neighbouring groups of tumours he has clearly defined. They spring from a connective-tissue basis, and may therefore be found in all parts of the body where areolar tissue, bone, cartilage, skin, mucous membrane, and the cognate textures exist. In structure they have an affinity with the tissues from which they take origin, consisting of cells, and an intercellular substance freely traversed by bloodvessels. But they differ from the simpler homologous growths—such, for example, as a fibrous tumour—in that the development and increase of the cells predominate over the development of the intercellular substance, whilst the cells tend to assume distinct forms; and from the heterologous growths, the carcinomata, they differ in that there is a distinct formation of intercellular tissue, though of rudimentary type, and not a mere infiltration of cells among the pre-existing tissues. The cells present either, 1st, a fusiform or oat-shaped outline, when we have the spindle-celled sarcoma; or, 2d, they are more distinctly circular when we have the round-celled variety. Sarcomata with stellate cells and with large myeloid cells are found more rarely, and only in special situations.

Such tumours have long been occasionally noticed as occurring in the uterus. In his chapter on Fibro-plastic or Sarcomatous Tumours, Lebert[1] gave the history of a case, with microscopic drawings, where the disease sprang from the cervix uteri. Hutchinson recorded a case of intra-uterine sarcoma very fully in the Transactions of the Pathological Society of London in 1857,[1] under the designation of Recurrent Fibroid Tumour of the uterus. In the following year[2] another case was brought before the same Society, under the like designation, by Callender; and the same cases, with a more complete clinical history, are related by West,[3] who had the care during life of the second patient, and to whom belongs the merit of giving the affection a place, though not clearly defined, among the diseases of the uterus. In his lecture on Sarcoma,[4] Virchow described its occurrence in the uterus from instances that had come under his own observation, and gave reference to previously-recorded cases. But it was not till after the appearance of Gusserow's essay[5] on Sarcomes of the uterus that the subject received due attention, and gynecological literature began to be enriched by the observations of Hegar,[6] Winckel,[7] Spiegelberg,[8] Chroback,[9] Leopold,[10] Paul Grenser,[11] Ahlfeld,[12] and especially by a more elaborate thesis by Kunert, an abstract of which is given in the "Archiv für Gynaekologie"[13] in 1874. So late as 1873 the subject is meagrely treated by Barnes;[14] and it is only in the works of Schroeder[15] published last year, and in the last edition of Thomas,[16] which came into my hands two days ago, that a distinct chapter is accorded to Sarcoma of the uterus. Let me add, that I have looked through the index of the Transactions of our own Society, and that of the Transactions of our younger but more prolific sister in London, without finding a reference to the subject, unless an interesting case recorded by Hall Davis[17] as one of intra-uterine Fibro-plastic Tumour belong to this category. Let this be an apology for bringing four cases under your notice, and offering a few remarks regarding them.

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[1] Physiologie Pathologique, ii. 154, 1845.

The first case is one where we have probably to do with a uterine myoma or fibro-myoma undergoing sarcomatous transformation.

CASE I.—*Probable Fibro-myoma—Repeated Removal and Recovery—Development of Myo-sarcoma—Removal and Recovery—Development of Sarcoma—Removal of Portion, and Death of Patient.*

I first saw Mrs B., a widow, of 46 years of age, mother of several children, in consultation with Dr David Gordon of George Square, on 12th July 1870. She was pale from great anæmia, with a weak, quick pulse, and complained of great pelvic distress. The evacuation of the bladder especially was accomplished with great distress and difficulty. A tumour, the size of a child's head, was found rising above the pelvic brim, firm and fixed, and having all the characters of a uterine fibroid. On vaginal examination the pelvic cavity was felt to be completely occupied by a soft, fleshy, elastic mass, which the combined examination showed to be of a piece with the supra-pubic tumour, and which occupied the whole anterior wall of the uterus. The os uteri could, with difficulty, be detected high up towards the promontory of the sacrum; and the pelvic excavation was so completely blocked up that catheterization of the bladder was almost impossible. The loop of a straight chain-écraseur was applied as high up on the growth as possible, and a nearly circular portion was removed, measuring 4½ inches in diameter, and 2½ inches in thickness. On section it presented a uniformly smooth surface of pale pinkish colour, with two islands in it presenting the familiar cotton-ball structure, and clear white glistening aspect seen on section of an ordinary fibroid tumour of the uterus, and separated from the softer surrounding tissue by a connective-tissue capsule. The larger part of the tumour was composed of fusiform nucleated cells, with an intercellular matrix having a fibrillated appearance, and running for the most part in small sections in parallel directions. It presented a marked contrast with the irregular wavy arrangement of the fibres and connective-tissue corpuscles seen in the fibromatous nodules. In some places among the bundles of spindle-cells there were to be seen rounded nucleated cells, rather larger than white blood-corpuscles.

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[1] Vol. viii. p. 287.

[2] Vol. ix. p. 327.

[3] Lectures on the Diseases of Women, 3d ed., 1864, p. 328.

[4] Die Krankhaften Geschwulste, B. ii. p. 350, 1864-65.

[5] Archiv für Gynaekologie, B. i. p. 240, 1870.

[6] Ibidem, ii. 29, 1871.

[7] Ibidem, iii. 297, 1872.

[8] Ibidem, iv. 344, 1872.

[9] Ibidem, iv. 549.

[10] Ibidem, vi. 493, 1874.

[11] Ibidem, vi. 501, 1874.

[12] Ibidem, vii. 301, 1875.

[13] Ibidem, vi. 111.

[14] Clinical History of the Diseases of Women, p. 825. At p. 752 he quotes Hutchinson and Callender's cases of "recurrent fibroid."

[15] Krankheiten der Weiblichen Geschlechtsorgane, p. 284. He gives the history with drawings of two cases observed by himself.

[16] Practical Treatise on the Diseases of Women, 4th ed., p. 539, 1875. Has met with four cases, but their histories are not recorded.

[17] Transactions of the Obstetrical Society of London, ii. 17, 1860.

The patient's earlier history, as Dr Gordon informed me, presented nothing remarkable; her elder children having been born without any difficulty. But on 30th November 1865 the birth at the full time of her sixth child was impeded by a soft elastic swelling, which was then, for the first time, discovered in the right side of the vagina, and which necessitated the use of the long forceps, which was applied by Sir James Simpson, Dr Keiller also being present. Within a year, on 22d October 1866, having again carried a child to the full time, the delivery had to be terminated artificially by Dr Keiller, who turned and extracted the infant by the feet. Except in connexion with her labours the patient had not complained of special distress arising from the tumour; but in the following year it began to be attended with a copious serous discharge, and to produce pain and considerable difficulty in micturition. In June 1867, accordingly, a portion of it was removed with the écraseur by Sir James Simpson, with the effect of relieving the patient for a time from her discomfort. By November 1868, however, her distressing symptoms had returned to such a degree, that removal of another slice of the growth was attempted. The tumour at that time must have been very dense and tough, for though the portion removed was of smaller dimensions than that which came away in my hands subsequently, Dr Gordon tells me that the chains of two different écraseurs with which the tumour was seized snapped asunder, and Sir James had to cut through the constricted part with strong scissors. Again the patient was for a time relieved; and again the mass, which had been felt in the abdomen, seemed to lessen considerably. But on 3d February 1870, the vagina having again become blocked up, a third lobe of the tumour was removed; and on 24th February a fourth portion. The relief experienced by the patient lasted till the following year, when she was fain to submit to an operation for the fifth time—the amputation in July 1870 of the mass I have described.

After the operation the patient rallied speedily and satisfactorily, and the relief afforded by the operation tided her over three more years. In the spring of 1874, however, her old symptoms began to trouble her; the watery discharge became more profuse than ever; the dysuria returned; and in addition, in April she had a severe attack of menorrhagia, and her health began to deteriorate under repeated uterine hæmorrhages. I saw her again in October 1874, and found her greatly reduced in strength, with a waxy colour, and anxious expression of countenance, more pronounced than one sees in cases of mere anæmia or marasmus. There was no marked elevation of the fundus uteri above the pubes; but the vagina was occupied by a tumour the size of the fist, with a smooth surface, and having a soft, sodden feeling, bathed with a copious faintly putrescent discharge, and bleeding easily when somewhat roughly handled. We deemed it desirable to give her the chance once more of a temporary relief and rally, and on 31st October, with the écraseur, I removed the projecting mass, Dr Ogilvie Will of Aberdeen being also present. The chain of the instrument cut through with great ease, and with something like the sensation one experiences in cutting through a cauliflower excrescence. The structure of this last amputated mass much resembles that removed in 1871, except that there are now no traces of fibroid tissue; the round cells are more numerous, and are found running in parallel rows in a fibrillated but gelatinous-looking matrix, and the spindle cells are plumper and shorter. There was a slight tendency to hæmorrhage from the raw surface, which was controlled by perchloride of iron. The patient seemed to do well for two days, but was attacked with acute and extensive phlebitis in both the lower extremities, and on 13th November she died. A post-mortem examination was not obtained; but when I saw her shortly before her death, I found in the vagina, and springing from the wound surface, a fresh sarcomatous sprout about the size of half the fist, having the feel, and when exposed by separating the labia, the exact appearance, of a mass of proud flesh.

This, as I have said, appears to me to be a case where, after removal of a large portion of a myome or a fibro-myome, there has sprung up a neoplasm, which became more and more decidedly sarcomatous in its texture.[1] But the greater proportion of cases which have been recorded are sarcomata *ab origine*, and they present themselves under one or other of two forms. Either, 1st, they are circumscribed, and more or less solid bodies projecting polypoidally from some spot on the inner surface of the uterus; or, 2d, they are more diffused and soft, and spread along the uterine mucous membrane. The next two cases are illustrations of the former.

CASE II.—*Intrauterine Sarcoma—Removal and Recovery on two occasions—Death after five years and four months—Autopsy.*

Mrs M., aged 72, married 46 years, and mother of five children, with some intercurrent miscarriages, was placed under my care in the year 1869 by Dr Ritchie of Glasgow, who had instructed her to wear a vaginal ball-pessary, kept in position by a wire fixed to an abdominal band, to give relief from downbearing symptoms, from which she had suffered for many months previously. When I first saw her she was a well-nourished healthy-looking old lady, and I found, on inquiry into her symptoms, that besides having had for a long time a feeling of pressure and discomfort in the pelvis, she had more recently been surprised to notice occasional escapes of blood, menstruation being for many years at an end. On making a vaginal examination, I found the genital canals moist and relaxed, the os round and patulous, the cervix and body enlarged and softened; and when the fundus was pressed down with the left hand above the pubes, the finger of the right passed easily and painlessly into the cavity of the uterus. Within the os internum, I could thus feel very distinctly a soft polypus, which I judged to be of the size of a walnut. Not being prepared to attempt its removal, I prescribed ergot, with the view at once of checking the attack of hæmorrhage, which had been the immediate occasion of my present visit, and of inducing uterine action to favour the further descent of the intrauterine body. Two days afterwards the hæmorrhage had ceased, but, on examination, I found the os firmly closed, and the cervical canal quite impervious to the finger. An examination with the sound shortly afterwards, however, showing me that the uterine cavity was enlarged to three inches in length, and occupied still by the foreign body, I dilated the canal with sponge-tents, and partly with forceps, and partly with the finger-nail, broke down and removed the morbid growth which I found springing from high up in the cavity. It was too soft and friable to be removed in any other way, and I applied solid nitrate of silver to the ragged basis. She had no bad symptom after the operation, and for some months was in the enjoyment of good health. About eight months subsequently, however, there was a recurrence of the escape of blood; and she began now to complain more of leucorrhœal discharge, and as the os relaxed occasionally when the bleedings came on, so as to permit the passage of the finger into the uterine cavity, a new mass could be detected pressing into the os internum. In August 1870, exactly a year after the first operation, I again dilated the canal, and removed, with the assistance of Dr Munro, into whose care the patient was about to pass, a portion of the diseased mass the size of an egg; but it was felt to have acquired a more extensive attachment along the back wall of the uterus, and its complete extirpation, because of the bleeding set up in the very friable substance, could not be effected. Again, for nearly a year, she enjoyed comparatively good health, although the thin, pale, watery discharge soon reappeared, and by-and-by became very profuse; and for the relief of her pressure symptoms, which were sometimes accompanied with frequent micturition, and even retention, she had to wear a Hodge's vaginal pessary. After this time the hæmorrhages recurred, and the profuse watery discharge became offensive. Early in 1872, I saw her along with Dr Munro, when the uterus was found considerably enlarged, the orifice freely open, and occupied with a portion of the neoplasm hanging through in a sloughy condition, and giving rise to a profuse dirty discharge, having such a fæcal odour as to give rise to the suspicion that a fistulous communication had formed between the rectum and vagina. Her general health was now greatly impaired, her pulse weak, and her skin waxy, and it seemed that any attempt to remove more of the tumour might be likely to lead to a fatal collapse. Means were therefore used simply to keep up the patient's strength, and a portion of the tumour having been expelled, she rallied to a remarkable degree. From time to time, however, as Dr Munro has informed me, she suffered from increase of the watery discharge, and under a good deal of sickness and constitutional distress, the os uteri became more expanded, and pieces of the tumour were thrown off, sometimes as large as a pear, and invariably with some degree of hæmorrhage. These attacks came on every six or eight weeks; and on several occasions Dr Munro removed the protruding mass when it was hanging into the vagina. The strength was gradually undermined, and the blood disintegrated; and in the end of 1873, and again in 1874, she had attacks of phlebitis in both legs, but most severe in the left. She sank at last, and died on 29th November 1874, five years and four months from the date of the first detection of the sarcoma.

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[1] Not unlike what occurred in a case recorded by P. Müller. Archiv für Gynaekologie, vi. 126, 1874.

Dr Munro was allowed to remove the uterus, which he kindly gave me. It is enlarged in size, and presents the general appearance of a uterus at the fourth month of pregnancy, except that the os is expanded to the size of a two-shilling piece, and occupied by the projecting growth. It measures from fundus to os externally 5″ 9‴, from side to side at the level of the Fallopian tubes 5″ 5‴, and from before backwards 3″. The Fallopian tubes are seen to be enlarged to a corresponding degree, each having a thickness of from half an inch to 1″ 1‴, and from their free fimbriated extremities there project rounded masses, having the appearance of the thrombus projecting from a small vein into a larger trunk. That on the right side pouts out to the extent of half an inch from the tube. They are evidently of the same nature as the growth protruding through the os uteri, though somewhat paler in colour. On cutting open the anterior wall of the uterus the morbid mass was found adhering to the whole surface, but not so firmly as to hinder its being easily detached with the handle of the scalpel. In the two lower thirds of the posterior wall, however, the union is much more intimate. The walls of the uterus were everywhere thinned to a remarkable degree. In the middle of the anterior aspect they measure only from 1″ to 2″ in thickness. An incision running at right angles to this at the level of the Fallopian tubes cuts through a portion where the wall is thicker, but only because of the development of a few sarcomatous nodules within the layers of the muscular fibres. The mucous membrane is smoothed out, and presents a cribriform appearance from the processes thrown into it from the surface of the tumour, and a scraping of it placed under the microscope shows it to be deprived of its epithelial covering. In the posterior wall the sarcomatous tissue has firmer hold of, and passes deeply into the whole thickness of the walls—so much so, that at three spots nearly at the level of the os internum it has grown right through the muscular wall, and formed slight projections on the peritoneal surface the size of a half-cherry, covered with a thin investment of serous membrane. The walls of the Fallopian tubes are similarly atrophied till there seems to be little more than the serous covering left. On microscopic examination the tumour presents many nucleated cells, both round and spindle shaped; the latter in by far the greater proportion. But they are for the most part filled with fatty granules and molecules, and the fibrillated tissue around them is also dimly granular.

This case is remarkable clinically, because of the frequency and extent of the sloughy masses which were expelled; and pathologically because of the manner in which the growth, while rooting itself specially in one part of the uterine wall, yet laid hold eventually of its entire mucous membrane, not only in the body but also in the cervix. In its invasion of the Fallopian tubes, and finding in them a new seat of development, the case is, I believe, unique.

CASE III.—*Intrauterine Sarcoma—Inversion of the Uterus—Amputation with Ecraseur—Death—Description of Tumour.*

J. P., aged 41, unmarried, came from the country into my ward, in the Royal Infirmary, on 2d September 1875. She was a stout, well-built, anæmic-looking woman, and with a somewhat worn expression of countenance. She complained of being very weak, and becoming increasingly weak, from the occurrence of frequent bloody discharges. The first had appeared at a menstrual period, ten months previously, when the amount of flooding alarmed her; and to the recurrence of her courses at shorter intervals than usual and in increased amount she attributed her weakness. During the intermenstrual periods there was a copious leucorrhœal discharge, which had only latterly begun to be offensive. She had no pain till two weeks before her admission, when she began to have distress in the pelvis, more particularly felt in the bladder, micturition becoming difficult, and then impossible, so that for three or four days her only relief was obtained by the use of the catheter. Since then she passed urine of herself but with some difficulty and in rather small amount. She stated that some months before she had swelling of the left leg from the knee downwards, and both legs were occasionally swollen at night. The urine had a deposit containing pus and a few blood-corpuscles, but was free from albumen. Palpation over the hypogastrium elicited some degree of tenderness; otherwise nothing unusual was to be felt through the abdominal parietes. On examination per vaginam, a large tumour was felt distending the vaginal walls, and filling up the pelvis. It was rough and ulcerated at its lower aspect; the sides felt smooth and fleshy, and in the roof of the vagina it narrowed towards a pedicle, which seemed to come through the os uteri. Because of the pain and bleeding excited when exploratory attempts were made with the sound, and by combined internal and external examination, the exact relation of the morbid growth to the uterine cavity could not at first be ascertained. The patient was ordered to be kept at rest, to use ergot and a tonic internally, and to be syringed with an astringent and disinfectant injection. Her general condition not improving, and occasional attacks of vomiting coming on, on 18th September I had her brought under the influence of chloroform in the presence of Dr Edmonstone Charles of Calcutta, Dr Halliday Croom, and the resident physician and clerks, with the view of more fully examining, and, if possible, removing the growth. It was now discovered that the fundus uteri could not be touched through the abdominal walls, that the larger globular portion of the body occupying the vagina was of softer texture than the pedicle, which came through the circle of the os uteri indeed, but felt as if it were attached to the cervix within a few lines of the orifice. It was impossible to get through the os with the finger except at one point, but the absence of the fundus at its usual level leading me to suppose the uterus might be inverted, by means of the sound I ascertained that such was the case. The tumour, rather firm to the feel as it was packed in the vagina, proved to be too friable to be easily seized with forceps or vulsella. With the four fingers of the right hand, I broke off about a half of it, and then the remainder, with the completely inverted uterus, was easily brought down to the vulva. The loop of a chain-écraseur was applied around the base, but as it involved the whole of the fundus the amputation had to be carried through a plane nearly at the level of the orifices of the Fallopian tubes. Notwithstanding that it was slowly cut through, there was some jetting of blood from the wound surfaces, and as the patient was already much reduced by loss of blood, I brought the raw margins together with four metallic stitches, which completely checked all further hæmorrhage. The patient did not seem to be much affected as to her general condition immediately after the operation, and on the following day seemed to give fair promise of recovery. On the third day, however, there was a rise in the temperature, and in the evening she died very suddenly, and with some symptoms, such as breathlessness, that led Dr Croom (I was myself out of town and did not see her) to suspect that she might have had a pulmonary embolism. A post-mortem examination was not permitted.

On examination of the amputated fundus uteri, with the adherent growth, it is easy to see how intimate is the union of the latter with the uterine wall. The union, indeed, is so intimate that one cannot tell where the transition takes place from the tissues of the neoplasm to those of the uterus. The growth is found on microscopic examination to be composed of large spindle-cells, each with a clear nucleus, lying close together in parallel bundles, and close up to the serous coat of the uterus some of the well-marked bundles of spindle-cells can be traced among the strata of muscular fibres and connective tissue of the middle coat.

The most interesting feature in connexion with this case is the inversion of the uterus. It is now the fourth case of sarcoma uteri with this complication. The first case on record[1] will be found in the Lectures on Pathological Anatomy by Dr Wilks (p. 404), published in 1859,[2] but the clinical history is wanting. The next case was brought under the notice of the Berlin Obstetrical Society, in January 1860, by Langenbeck;[3] the patient died without being operated on, and being already very much reduced, the death seemed to be hastened by the bleeding caused by the examination made to clear up the nature of her malady. In the third case, by Spiegelberg,[1] the entire inverted uterus was amputated with the écraseur, and the patient died in thirty hours with acute peritonitis. The frequency of inversion of the uterus as a complication of sarcoma uteri is surely somewhat remarkable. If we add[2] to the thirty-nine cases collected by Kunert[3] the case above referred to by Wilks, the two given by Schroeder in his text-book, one by P. Müller,[4] one by Ahlfeld,[5] and the four which form the groundwork of this communication, we have a total of forty-eight cases of uterine sarcoma, and in four there was complete inversion of the uterus. Besides that Spiegelberg notes a tendency to inversion in two of his other cases, the fact that in one out of every twelve cases of intrauterine sarcoma, the organ had become completely inverted, is sufficiently striking, for we find no such frequency of inversion in the cases of the common intrauterine myomata. But this complication becomes more remarkable when we consider that all the three patients with inversion, whose history we know, were nulliparous females, or, if one of them (Langenbeck's case) had given birth to a child, it was twenty-three years before the occurrence of the sarcomatous inversion. There are two points, however, in connexion with the development of intrauterine sarcomes in which they differ from the common myomes, and which prepare us to expect the production of inversion. In the *first* place, the sarcoma springing, say, from the fundus uteri, is in intimate union with the walls from which it grows, whereas the myoma is surrounded by a capsule, and so more easily separable from the bed in which it lies. The result of this difference in the relation of the two kinds of tumour to the tissues where they originate is, that when the uterus begins to contract for the expulsion of the neoplasm, the sarcoma drags down with it its seat, in the wall of which it forms an integral part; whereas the myome gets gradually detached from its loose connexions and becomes pediculated, and perhaps is at last expelled from the cavity of the uterus. Doubtless, also, the condition of the layers of the muscular fibres between the neoplasm and the serous surface differs; in my case certainly there must have been some degree of paralysis at the site of the tumour, whilst a myome in the same situation would probably have been covered with a layer of healthy muscular fibres, which would concur equally with the rest of the uterine muscles in pressing the tumour towards the cavity. But, in the *second* place, there is in these sarcomatous cases an unusual relaxation of the walls of the genital canals, more than is to be accounted for by the moistening with the frequently profuse discharge. I was particularly struck with this in the second of the cases which I have related. There were times when though at the first touch of the finger the os was too small to admit it, yet on continued pressure it yielded and opened up; and, as I have stated, there were times when it opened freely and widely to permit of the escape of sloughy portions of the tumour. It was as if a relaxation and dilatation took place at those times when the uterus was making efforts for the birth of the neoplasm, parallel to the relaxation and dilatation that accompany the contractions of the uterus in ordinary labour. And it is to be remembered that that kind of vital dilatation is not confined to the cervical canal. Though the patient whose case is under discussion was unmarried, and the vulva in a virginal condition, so that the full examination could only be conducted after she was anaesthetized, it was remarkable to what an extent the genital orifice became dilated during the operation, so that first three fingers, and then the whole hand, could be passed into the vagina without lacerating the mucous membrane. This point was the more impressed upon my mind from the circumstance that less than three weeks previously I had seen, with Dr Brotherston of Alloa, a case of a large fibro-myomatous polypus coming down from the interior of the uterus of the size of two fists, and hanging by a somewhat narrow neck into the vagina. The patient in that case also was an unmarried female of about 40 years of age, and after I had succeeded in detaching the tumour from its uterine connexion by torsion, I was almost baffled in my attempts to extract it through the firm and unyielding vulva, and only succeeded at last by cutting the tumour into pieces within the cavity, the patient being kept under the influence of chloroform for upwards of two hours. The singular dilatability of the canals, and the intimate connexion between the sarcoma and the part of the wall to which it is attached—at once giving it a strong purchase upon its seat and impairing the action of the muscular fibres in that portion of the wall—seem to me to afford a fair explanation of the marked proclivity of the sarcomatously affected uterus to become inverted.

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[1] Apparently overlooked by Kunert, etc., though it is referred to in a note in Virchow's Onkology.

[2] Under the heading "Recurrent Fibroid Disease." In the recent edition—Lectures on Pathological Anatomy, by Wilks and Moxon, 1875—it is given, p. 561, under the heading "Sarcoma."

[3] Monatsschrift für Geburtskunde, xv. 173.

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[1] Loc. cit., p. 351.

[2] I leave out of account cases, such as are recorded by Leopold and P. Grenser, of sarcoma springing from the cervix uteri, for in such inversion of the organ was not likely to take place. Perhaps some of Kunert's thirty-nine cases are instances of cervical sarcoma.

[3] Loc. cit., 116. [4] Archiv fur Gynaekologie, vi. 125. [5] Ibid., vii. 301.

In the next case we have an example of the diffuse sarcoma.

CASE IV.—*Intrauterine Sarcoma—Patient still under Observation.*

Mrs C., aged 45, admitted to Ward XIV., 4th October 1875, has been married twenty-five years, and is mother of ten children, the eldest born twenty-three, the youngest nine years ago. She had always menstruated regularly and enjoyed good health till eighteen months ago, when she began to suffer from floodings. Within the last four months the pains, which remind her of labour pains, have begun from time to time to distress her. Four weeks ago one of her floodings set in, and continued for seven weeks, large clots sometimes escaping. For a fortnight the bleeding ceased, but she had then a profuse watery fœtid discharge, and a week before her admission the bleeding returned. She was greatly reduced in her general health, with a weak, quick pulse, and a cachectic expression of countenance. There was nothing to be felt on abdominal palpation. On vaginal examination the uterus felt heavy; the os was soft and patulous, and easily admitted the tip of the finger as far as the os internum, at which a substance with the feel of a soft clot could be touched. The cervical canal having been further dilated with a tent, the cavity of the uterus was felt greatly expanded; the front and back walls were covered with soft, ragged, irregular patches of tissue, a larger and more prominent portion of which grew downwards from the fundus. But when I attempted to make a combined external and internal examination, the walls of the uterus gave me so much the impression that they were ready to tear, and there began to flow from the already enfeebled patient such a stream of blood, that I was fain to desist, and to arrest the hæmorrhage by a free application of perchloride of iron into the uterine cavity and the introduction of a vaginal plug. She soon recovered from the effects of that bleeding, and went home, as further operative interference gave no hope of any good result.

She has had less pain since the uterus was dilated, and but little hæmorrhage; but the watery discharge is more profuse and very offensive. A small fragment of the excrescence removed with the finger-nail shows the characteristic appearance of the spindle-celled sarcoma. Some large epithelial cells with double nucleus which appear in the field of the microscope may have been derived from the vaginal canal during the abstraction of the minute fragments of tissue. At least there were no similar groups of cells collected in interstices of connective tissue, such as we see in cases of carcinoma.

The narration of the cases, with the epicritical remarks, has extended to a greater length than I had anticipated. What further general observations I have to add must therefore be brief.

Etiology.—My cases do not confirm the impression made by statistics already published, that nulliparity favours the development of sarcoma uteri. In one of them, as I have said, the patient was unmarried; but the other three patients were married women, who had each been mothers. As for age, they were all approaching, or had passed, the menopause. The youngest was 41, the oldest 72 years of age at death. Their social circumstances were comfortable, even the hospital cases not being drawn from the poorest class of society; and so far as could be ascertained, their family history was good. On looking the ward statistics for the last four years, with the view of comparing the proportion of sarcomata to the more common uterine neoplasm, I find 20 cases of myoma or fibro-myoma, and 32 cases of carcinoma, 2 of which were intra-uterine, whilst there were only the two cases above recorded that could be regarded as sarcomatous.

Symptoms and Diagnosis.—In most cases these growths give rise to a marked degree of *hæmorrhage.* The usual monthly discharge becomes exaggerated, or bleedings come on in the intermenstrual interval, or they are set up in women who had passed the menopause. These hæmorrhages are easily intelligible when we bear in mind that the tumours are always vascular, sometimes channelled with large and thin-walled bloodvessels; and that from the softness of their texture they are easily broken down. But, in certain cases, the tendency to hæmorrhage is not pronounced. There is usually, however, a marked degree of *leucorrhœal discharge.* The succulent and sometimes suppurating surface of the tumour itself, and the thickened and expanded mucous membrane lining the cavity in which it lies, furnish a free secretion of a pale, rice-watery, or more yellow fluid, which, in any case, may for a time disappear, though it is almost never absent (only in Leopold's case, where the tumour developed in the cervix) throughout the entire history of any case. This discharge will, for a long time, be odourless, but it acquires something approaching to the disagreeable odour of the cancerous discharges in the more advanced stages of the disease, when fragments of the tumour are squeezed into the vagina, and lie within the canals till they decompose. We have seen that the mischief may develop in women who have borne children; but after it has appeared, the *reproductive function* of the organ is at an end. For I know of no case of the concurrence of pregnancy and sarcoma uteri, such as we sometimes meet with in myoma or carcinoma: probably from the circumstance that the sarcomatous growths almost always affect the interior of the uterine cavity. There has been some diversity of opinion as to whether this affection is attended with *pain* or not. My own impression is, that pain is not a usual symptom of it, any more than we can speak of pain being a usual symptom of a fibrous polypus. In both cases we may have suffering from the enlargement, or from intercurrent inflammation in the uterus or around it, or, what is still more common, we may have pain from the muscular contractions set up from time to time in the walls of the uterus for the expulsion of the neoplasm. But all this is different from the uncertain, unprovoked, sharp stinging pain to which a cancerous patient is subject. More particularly I have been struck with the difference between the pain complained of by these patients who are the subjects of intrauterine sarcoma, and the intense, paroxysmal attacks of suffering, coming on sometimes[1] with a marked degree of periodicity, which Sir James Simpson used to point out as characteristic of intrauterine cancer. It was the absence of this symptom, which I had seen in several instances, that first led me to think that, in the case No. IV., we should find, as we did, not the hard, rough surface of an epithelial, but the soft, velvety, friable surface of a sarcomatous growth. In addition, there is *disturbance* in the functions of *neighbouring organs* differing in degree in different cases according to the size and situation of the neoplasm and its bed. Sooner or later the repeated losses of blood or the continued drain of watery discharge begin to tell on the patient's general health; and when the disease has advanced towards its final stages, it usually develops in the sufferer a condition of *general cachexia.*

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[1] See an illustrative case in a Clinical Lecture by Thomas, in the American Journal of Obstetrics, v. 703, 1873.

The physical examination of a patient with such a train of symptoms would discover some degree of uterine enlargement, varying according to the size of the morbid mass occupying its interior. It is rarely so large as to form an abdominal tumour, except where we have to do with a myo-sarcoma, or a fibroid tumour undergoing sarcomatous degeneration, when we may find a suprapubic mass of considerable dimensions, with the vascular bruit and other characteristics of such growths. Where the growth springs from the cervix, or the uterus has become inverted, a polypoidal body will be felt occupying the vagina. The uterus, though enlarged, remains freely movable, unless the tumour be so great as to cause some impaction in the pelvis, or inflammatory adhesions have formed among the pelvic organs—conditions which are both comparatively rare in the true sarcoma. The sound will pass without difficulty into the interior of the uterus, and detect the extent of increase of the cavity and the presence of a foreign body in which bleeding is easily excited. But it is only when the canal is dilated with a tent—and, as I have shown, the textures are so soft and expansible that a single good-sized sponge-tent may suffice for the dilatation—that the size, consistence, and attachments of the new growth can be discovered, and that a fragment of it can be obtained for microscopical investigation. It is in this way alone that an exact diagnosis can be made out.

Prognosis.—The comparative degree of malignancy to be attributed to this neoplasm may be gathered from the significant circumstance, that West treats of it in the same lecture with fibrous polypi, whilst Barnes gives it a place among the cancers. Although the disease shows a distinct tendency to remain localized in the uterus—these cases being but few where secondary sarcomata were found in other organs—there seems to be only one recorded case (Winckel's first) where removal of the growth was followed by a radical cure; and it is to be noted in regard to it, that it was a polypoidal myo-sarcoma, and not a pure sarcoma, and that the patient's history does not extend beyond two years from the date of operation. According to Kunert, "in 14 out of 30 cases death ensued within a year after the first appearance, in 1 after 2 years, in 3 after 4–6 years; in 3 cases the patient was still suffering after 2 years; in regard to the remaining cases, partly observation is awanting, and partly the patients enjoyed tolerable health under continuous medical treatment." One case is recorded[1] where an

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[1] Rabl-Rückhard in Beiträge zur Geburtshülfe, i. 76, 1872.

Case 11.—J. S., æt. 6, had suffered from the disease for ten months, the symptoms following an injury of the joint. For eight months he was treated with extension and the long splint, and about two months previous to admission a large abscess formed over the outer aspect of the thigh and burst, discharging many ounces of pus. The signs of suppuration of the joint being well marked, an incision was made on the 12th of July 1872, and the diseased bone removed. The head and a portion of the neck of the femur were affected, so that the section was made through the great trochanter. A portion of the floor of the acetabulum was affected and was gouged away. This patient made a slow but good progress, and he was dismissed cured with a useful limb on the 15th of May 1873. Fig. 32 is taken from a photograph of this patient.

Case 12.—M. C., æt. 28, had suffered from symptoms of the disease for six years, and had been treated in various ways. Suppuration of the joint took place, with great pain and much constitutional irritation, so on the 30th of September 1872, I removed what remained of the head and neck of the femur by sawing through the bone immediately below the trochanter. The head of the femur was partially anchylosed to the acetabulum, which rendered the operation more difficult than usual. The acetabulum was enlarged and carious at several points. The patient improved after the operation, and for nine months his extension for three months, but abscesses formed in the groin and thigh, and caused great constitutional irritation. On the 9th of January 1873 the abcesses were opened, and the head and neck of the femur were removed by sawing through the bone close to the great trochanter. The acetabulum was healthy, except at one point on its floor, and this was scraped with the gouge. His progress was slow but good, and he was dismissed to the Convalescent Hospital in June. In August of the same year, about eight months after the operation, he was examined and found to have a perfectly useful limb. The limb was about three inches short, but freely movable at the hip and other joints, and the wound was soundly healed.

Case 15.—D. M'F., æt. 20, had suffered from symptoms of the disease for about one year, and had been treated by the usual means without success. Signs of suppuration were present, and deep fluctuation could be felt over the joint. On the 18th of March 1873, an incision was made, and the head of the femur removed, together with a small portion of the lip of the acetabulum, which was also diseased. The entire cartilage covering the head of the femur was destroyed, and the bone exposed and carious. His recovery was good, and he returned home on the 29th of June with a movable and useful limb, and with the wound healed. Two years after the operation, photographs of this patient were taken, It is now more than a year since a small quantity of this plant was brought to Paris by Dr Coutinho; and Professor Gubler, after experimenting with it at one of the hospitals of Paris, declared that the effects of jaborandi were extremely remarkable, and the evidence irresistible. Owing to the difficulty of procuring this drug, little of it reached this country till the beginning of the present year. It was on the 5th of January that I received my first supply, and I believe I received the first packet that came to Scotland. I lost no time in making an experiment, and found that the drug was possessed of remarkable physiological properties. Since that time, I have received sufficient to enable me to ascertain fully its physiological, and to a certain extent its therapeutic, properties. It is not my intention, in this paper, to detail at length the various experiments I have made with this medicine. I shall merely give the general results, which are sufficiently remarkable to interest the members of this Society.

An infusion, prepared from one drachm of the bruised leaves, when swallowed, produces the following results:—About twenty minutes after the infusion is swallowed, salivation commences, and in a few minutes afterwards the mouth is literally flowing with water. This continues for four or five hours, and during that period, from ten to sixteen ounces of fluid may be easily collected. The infusion is acid, has a peculiar and characteristic odour, a mawkish but not unpleasant taste, and in doses corresponding to one drachm of the leaves, I have never found it produce any nausea. I have invariably used the strained infusion, and am prepared to show in this paper that the strained infusion possesses all the activity of jaborandi. Notwithstanding the large amount of fluid secreted by the salivary glands, it produces no uneasiness in these glands. Simultaneously with the flow of saliva, the perspiration appears on the forehead and over the whole body; it extends even to the limbs, but is most marked on the trunk. These effects can be produced in a room with a temperature ranging from 54° to 56° Fahr., and without putting the patient to bed. It is not necessary to administer the infusion hot. The effects are produced even when the infusion is administered cold. The sweating continues for several hours.

There can be no doubt that this drug is possessed of remarkable powers as a sialogogue. Certainly I know no medicine, either in the Pharmacopœia or out of it, at all to be compared to jaborandi in this respect. Its power as a diaphoretic is also well established, although in this respect I did not find it so remarkable as I had anticipated from reading the accounts of Professor Gubler's experiments.

It has also been found that belladonna, or its active principle atropia, arrests the salivation produced by jaborandi, thus showing that in certain respects we have a true antagonism between jaborandi and belladonna.

I have not time in this paper to enter into the interesting question regarding the *modus operandi* of this remarkable drug; suffice it to say, it is in no way due to any local action on the salivary glands produced during the action of swallowing. It is well known that many vegetable substances, when chewed, possess the property of increasing, to a limited extent, the flow of saliva; and even jaborandi to a certain degree possesses the same property. The salivation, however, produced by this drug is something altogether independent of any irritation on the salivary glands, produced by being chewed or even swallowed, as the following experiment clearly proves:—I injected up the rectum the infusion prepared from one drachm of the bruised leaves, and found that salivation and perspiration came on as speedily and as effectually as when the same quantity was administered by the mouth, clearly proving that the active principle of jaborandi is first absorbed, and afterwards produces its remarkable physiological effects.

I shall now make some remarks regarding the active principle of jaborandi.

Various chemists have analyzed the leaves, and it is generally believed that its activity is due to an alkaloid, which some have called pilocarpin. The watery infusion of jaborandi is acid, but whatever be the acid contained in jaborandi, its physiological properties are not due to this acid. Neither are they due to the volatile oil contained in the leaves, and to which they owe their characteristic odour. For I have had prepared for me by J. F. Macfarlan and Co. of this city, a substance called pilocarpin, which undoubtedly possesses the physiological properties of the drug, and yet is not acid, and does not contain the characteristic odour of jaborandi. It is quite possible, for anything I know, that the acid and volatile oil may possess sialogogue or diaphoretic properties, for I am not aware that they have as yet been isolated; but it is clearly proved that there is a substance which can be separated from jaborandi, destitute of acidity and of the characteristic odour of that plant, and which possesses all the activity of the drug. This substance, called *pilocarpin*, is of a semi-fluid consistence, of a yellowish colour, and possessed of an agreeable odour. It is soluble in water, and is very active. One grain of this pilocarpin is nearly as active as one drachm of the leaves. Whatever be the active principle of jaborandi, it is something which is soluble in water, and even the *strained* infusion possesses all the activity of the drug. In this opinion, I differ from others who have experimented with this substance. There are many who believe that to produce the physiological effects of jaborandi, it is necessary to use either an alcoholic preparation, or to swallow the "dregs." The origin of this erroneous view is, I believe, traceable to Rabuteau of Paris, who, as the result of a chemical analysis of the leaves, says that they contain—(1.) A volatile principle; (2.) A part soluble in water, and perfectly flavourless; (3.) A part insoluble in water, but soluble in alcohol, and having a bitter flavour: and that by experiments he has come to the conclusion that it is the last of these three in which all the virtues reside. The same erroneous opinion occurs in an editorial article on jaborandi, published in the *British Medical Journal* for 27th February 1875, where we have these words; referring to the leaves of jaborandi, it is said:—"They contain a volatile principle and a tasteless substance, soluble in water, both of which are physiologically inert, the active properties of the drug residing exclusively in a bitter extractive, soluble in alcohol, but insoluble in water."

Mr Martindale of London fell into the same mistake, and swallowed, in addition to the infusion, what he called the "dregs." These "dregs" consist, to a great extent, of pieces of the stems and leaves, together with the petioles; and on account of the large amount of fibrous tissue contained in these dregs, irritation of the stomach and alimentary canal is caused by their being swallowed. In the *Pharmaceutical Journal* for 16th January, Mr Martindale relates an experiment in which he swallowed about fifty grains of the dregs, in addition to the infusion, and experienced nausea and vomiting. This was but the natural consequence of swallowing such an amount of indigestible materials. In the same article, Mr Martindale uses this sentence:—"The strained infusion, from what I hear, produces but little effect." In the same number of the *Pharmaceutical Journal*, I gave an account of an experiment performed with the *strained* infusion, and found it possessed of very remarkable effects indeed. The whole of my experiments have been performed with the strained infusion, and this substance called pilocarpin, which I have found to be so active in doses of one grain. It is freely soluble in water. These facts clearly prove that the active principle of jaborandi is soluble in water, and also that the strained infusion possesses all the activity of the drug.

Regarding the effects of this medicine on the temperature, pulse, and vision, experimenters are somewhat divided. My own experience leads me to believe that they are very small and insignificant. I have never found jaborandi affect much either the heart's action or the temperature of the body, and I have watched carefully for the disturbance of vision observed by Mr Martindale and others, but as yet have failed to observe it. I believe that many of the untoward results ascribed to jaborandi are due to the fact that "dregs" have been swallowed in addition to the strained infusion.

I have not had much opportunity of testing the therapeutic virtues of this drug, but I have tried it in several cases of fever, where the tongue was dry and the mouth parched, and by giving small doses of the infusion every few hours, I was enabled to restore the flow of saliva, and to keep the tongue and mouth moist. I believe that in this respect it will prove of considerable benefit in fevers and allied diseases. It has also been efficacious in removing pleuritic effusions. For this disease it will yet prove a most valuable medicine; for in a few hours, in addition to profuse perspiration, twelve or sixteen ounces of fluid may be withdrawn from the blood, with very little disturbance to the general system, which must tend in no small degree to promote absorption of fluid from the pleural cavities. I understand it has been found useful in diabetes. Several cases of this disease have been successfully treated with jaborandi by Professor Laycock.

Regarding the applications of jaborandi to the cure of disease, much clinical observation is still necessary before we can assert positively what are the therapeutic virtues of this remarkable drug.

The following may be regarded as well-ascertained facts regarding jaborandi:—

1. Its leaves are possessed of remarkable sialogogue and diaphoretic properties.
2. Boiling water is capable of extracting the most, if not the whole, of these properties.
3. A strained infusion seldom produces any untoward results.
4. On account of the large quantity of indigestible fibrous materials in the "dregs," these, when swallowed, must produce unpleasant effects.
5. A substance of a semi-fluid consistence, destitute of acidity and the characteristic odour of jaborandi, may be separated, possessing all the activity of the drug, and which is probably an alkaloid.
6. The dose will be found to be $\frac{1}{12}$ to $\frac{1}{4}$ grain of this alkaloid, and of the infusion as much as corresponds to five or ten grains of the leaves.

Before I conclude, I wish to say a few words regarding the botanical source of this remarkable drug—the more especially as, in a recent number of the *British Medical Journal*, Dr Robert Cory of Carlisle, in an article on this plant, mistakes throughout a leaflet for a leaf.

There is still considerable doubt as to the botanical source of jaborandi. Some maintain that it belongs to the Piperaceæ; whilst others, with more probability, that it belongs to the Rutaceæ. I believe that I am correct in stating that the name jaborandi is given by the Brazilians to various plants possessed of stimulating properties, and in a dictionary of Brazilian medicines, in Portuguese, by Dr Chernoviz, published in 1868, jaborandi is mentioned as the produce of Ottonia anisum, a plant belonging to the Piperaceæ. Dr Chernoviz states that this plant is possessed of sialogogue properties, and that from the root a tincture is made (one of jaborandi to eight of alcohol). From the parts of the plant I have examined, I may safely say that the jaborandi of Dr Chernoviz is not the drug attracting so much attention in Europe at the present day.

There is a plant at present in the market called "piper jaborandi," whose virtues I have not yet had time to investigate, but certainly it is not the *true* jaborandi. The leaves of this plant are very different from those of that shrub whose effects I have described in this paper.

I have not yet been fortunate enough to receive those parts of the plant which are sufficient to determine its botanical source; but from the parts of the plant I have received, I have come to the conclusion that it belongs to the Rutaceæ, or an allied order, and may possibly belong to the genus Pilocarpus. I have been able to determine the following facts:—

The leaf is compound imparipinnate. The leaflets are in four or five pairs, and are scarcely opposite. They are glabrous, smooth, brittle when dry, and somewhat resemble the leaves of the Prunus Laurocerasus. They are elongated, elliptical, emarginate, and slightly unequal at the base. The leaflets vary much in size and form, even on the same leaf; but the foregoing characters are possessed by all. The venation is well marked, and is very beautiful. There is a very prominent mid-rib, on the under surface, running along the entire length of the leaflet; from this mid-rib numerous veins proceed (at an angle of about 45°) towards the margin of the leaflet, and form loops and anastomoses within its margin. The veins are well seen on both surfaces, but are most marked on the under side of the leaflet. The petiole is cylindrical, flattened and thickened at the point of insertion into the stem. The distance from the stem to the first pair of leaflets varies in the specimens I have seen from two to five inches, the average distance between the pairs of leaflets being about two inches. The leaflets are nearly, but not quite, sessile, and are inserted on the upper surface of the petiole. They are thickly spotted with pellucid dots. These are well seen when the leaflet is held before a bright light.

I have also been enabled to examine specimens of the stems and small branches, and find that the bark peels off loosely, is very brittle, and produces a peculiar sensation when chewed. Between the bark and wood, there is abundance of crystalline matter, but I have failed to find any definite forms of crystals when examined under the microscope.

ARTICLE IV.—*Tumour of the Dura Mater.* By DR F. MOINET.

J. T., æt. 40, widow, admitted to hospital 5th September 1874, as a case of neuralgia and ozæna.

On admission she complained of intense frontal pain, was very anæmic, pupils dilated, appeared very depressed, and would hardly answer when spoken to.

On account of the intensity, duration, and want of intermission of the pain, its fixed position, especially over the left frontal region, the depressed and anæmic condition of the patient, and because it did not follow the course of any special nerve, it was evident that the pain was not due to any functional derangement, and it was thought in all probability to depend on syphilitic affection of the membrane of the brain as the seat of pain. The history of the case was not incompatible with this opinion.

She was therefore ordered the following prescription, after the bowels had been freely moved :—

℞ Potassii bromidi, . . . ʒiv.
Potassii iodidi, . . . ʒii.
Aquæ, ... ℥vi. M.
Sig., Cochleare magnum ter in die.

On the 21st of September 1874, she left hospital of her own accord, feeling quite free from pain, but still looking very anæmic. She was again admitted into hospital on the 21st September 1875, complaining of the same pain, only in a much weaker and more anæmic condition, and was even more depressed, but answered sensibly ; pupils dilated, but no paralytic symptoms, and she was so weak that she had to be carried to bed.

The same treatment was ordered as formerly. The pain, however, continued unabated, and she gradually sank and died on 30th October, without further symptoms manifesting themselves.

Post-mortem, 5th October, by Mr Hamilton, to whose kindness I am indebted for the following report :—

" The dura mater over the anterior aspect of the first and second frontal convolutions on the left side, for about the space of a crown piece, was thickened and adherent to the surrounding parts. The bone, at the point of adhesion, was eroded, and a small amount of yellow curdy purulent material was seen between it and the thickened dura mater. This thickening of the dura mater was produced by the effusion into it of a gelatinous inflammatory material, constituting a tumour of considerable thickness, pressing on the bone on the one hand, and on the frontal convolution on the other. To the latter it was firmly adherent, and the brain substance all around was extensively softened ; the bulb of the right olfactory nerve was also implicated in the tumour, and its consistence destroyed. The trunk of the nerve from this to its bifurcation on entering the cerebral substance was markedly atrophied. On microscopical examination it was found that the dura mater was infiltrated with pus-corpuscles, and that these in great part evidently resulted from proliferation of its connective tissue corpuscles. Several of these were growing fatty, constituting the first stage in the formation of compound granular corpuscles. The neighbouring cerebral substance, for about a quarter to half an inch in depth, was infiltrated with pus and compound granular corpuscles, and was consequently much broken down and destroyed. The fibres of the olfactory nerve were in a state of trophic atrophy. The only cause for this was the destruction of the olfactory bulb, clearly showing that certain at least of the trophic centres of the olfactory fibres are situated in the bulb. It is probable, from the nature of the inflammatory exudation, that its origin was to be sought in a chronic syphilitic affection."

This case is interesting, as showing that although the tumour of the dura mater was not large, or in any way interfering with the functions of the brain, it had by pressure on the bone, and consequent pain, caused a fatal result. The depressing effect of the pain interfering with nutrition, causing the marked anæmia and debility; the patient consequently sinking from inanition, as there was no other lesion found to account for the result. Also that at the spot where the pain was most intense was the tumour situated. So that while Dr Balfour has shown, by the interesting cases reported in the October number of this Journal, that obstinate trigeminal neuralgia is frequently a symptom of syphilitic cerebral disease at the base of the brain, this case shows that, in the absence of other symptoms, intense frontal neuralgia may be due to syphilitic disease of the dura mater at the seat of pain. It is also interesting to note the relief that followed the administration of the bromide and iodide of potassium, which is just what we may expect from the physiological action of these salts.

ARTICLE V.—*A Ready Method of Preparing Sections of Diseased Tissues for the Microscope.* By JOHN STEVENSON, M.B., Forfar.

IT is frequently desirable, indeed the present advanced state of morbid anatomy, properly so called, renders it absolutely necessary, that the microscope should be appealed to in forming a correct opinion of the nature of morbid processes which have caused any degree of doubt, interest, or anxiety to the general practitioner.

It cannot be supposed that every one, or even the majority of practical men, takes the trouble, even supposing them to have the requisite knowledge and the manipulative skill, to follow up cases in which a post-mortem examination has been made to their utmost limit by microscopic investigation; yet it may be assumed as equally certain, that if there existed a rapid and easy method of getting good and sufficiently transparent sections for microscopic inquiry, the practice of tracing the effects of new formations and altered nutrition on the component elements of the body, would become more general, and would be a source of interest and instruction to many. I therefore venture to think that a short account of a method I have lately used, and by which I have obtained with little trouble and expense, sections which are all that can be desired, will be of use in promoting the more general study of morbid histology, and in obtaining fuller and more minute details of exceptional cases of disease occurring in the routine of general practice.

Some time ago, it occurred to me, that a mixture of glycerine and tragacanth might be used for the purpose of imbedding tissues both recent and hardened. The mixture soon becomes solid and like a stiff jelly, even when little of the gum is used; but by varying the proportions of the gum and glycerine, I have been able to obtain a substance having the following qualities to recommend it for the purpose under consideration. It closely surrounds and even grasps any substance introduced into it before it solidifies. It cuts like cheese after standing eight or nine hours, and by immersion in methylated spirit for twelve or twenty-four hours, parts with its glycerine and becomes still harder and more easily sliced. It requires no apparatus, although the well of a microtome offers a decidedly easier way of cutting sections when the mixture is allowed to solidify in it, and less care is required on the part of the operator when doing so. The material is easily dissolved off from the sections by cold water with the addition of a little glycerine. The materials can be procured at any chemist's, and are inexpensive. The process described by Professor Rutherford[1] of freezing in gum solution is an admirable one, but ice is not always to be had, and the *modus operandi* is more complicated than that now indicated. Then the method of Stricker by imbedding in thick mucilage contained within a cone of filtering or blotting paper, which is hardened by placing it in spirit of various strengths, is not uniform in its results, unless some care be taken to watch the consistence at various times during the process of hardening; it is, moreover, frequently necessary to imbed the cone of gum in paraffin before proceeding to cut sections.

Very good sections of solid organs may be made by means of a Valentine knife by one who has practised it, but success in doing so is not invariable, even in the most accomplished hands, and the method is only applicable in some cases.

The method I am in the habit of pursuing, is to take two drachms of glycerine, and mix it on a slab or plate with one drachm and a half of powdered gum tragacanth. The piece of tissue or organ to be sliced is placed in a two dozen or smaller pill box, and the mixture lifted on a flexible knife and poured into the box. This quantity just fills a half ounce pill box. It is allowed to stand in a cool place for from eight to twelve hours, when, if not wanted for cutting, the bottom is taken off the box, and it is slit up the side with a pair of scissors, and a solid, somewhat elastic, cake, corresponding in shape to the interior of the box, is obtained, with the piece of tissue imbedded in it. This is placed in methylated spirit till required. The tissue should be placed near the margin from which it is to be cut (by the thickness of the fluid it is held in position and does not rise out of the surface), and the cutting is conducted in the usual way. It will be found best to moisten the surface by dipping it in spirit and wetting the upper surface of the razor or section knife with a camel's hair pencil dipped in spirit, so that the section may float as it is cut. When it is to be kept in spirit for twelve to twenty-four hours, the mass is better mixed in the following proportions:—glycerine ʒii., tragacanth powder, ʒi., gum arabic powder, fifteen grains; this is mixed and used as above indicated. When placed in spirit the spirit extracts the glycerine, precipitates the gum arabic, and gives a material which may be cut into very thin slices; this is well suited for tissues previously hardened in some fluid, as Muller's fluid, or in spirit. If the proportion of gum tragacanth to glycerine is less than one drachm of the former to two fluid drachms of the latter, the resulting mass adheres to the knife, is too soft, and when deprived of its glycerine it is friable and falls to pieces. As glycerine is a hygroscopic body, the compound should not be exposed to the air more than twelve hours; but if not soon sliced it should be placed in methylated spirit, where it will improve and all danger of softening will be avoided. The properties of the mixture are quite in accordance with the chemical nature of its constituents, for glycerine dissolves gums and resins freely, being at the same time hygroscopic; and tragacanth "has twenty-five times the power of ordinary gum arabic in communicating the property of viscosity to water (Bucholz)."[1] If the tissue has been previously in spirit it must be steeped for a few hours in cold water before imbedding.

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[1] See Outlines of Practical Histology, p. 48.

ARTICLE VI.—*Laceration of the Perineum.* By JAMES YOUNG, M.D., Vice-President of the Obstetrical Society.

(*Read before the Obstetrical Society of Edinburgh,* 17*th November* 1875.)

THE treatment of laceration of the perineum is one of the most important questions that can come under the notice of the obstetric surgeon. I propose to cite two cases, illustrative of the benefit of treatment by the interrupted suture, more to elicit discussion than for the purpose of bringing forward any new matter. The second case is almost unique in regard to the extent of the rupture, and likewise the result.

Case 1.—Some twelve months ago I was in attendance upon a lady in Maitland Street. It was her first confinement. The labour was protracted and difficult, requiring the use of the short forceps. I used very considerable strength in traction, but without the pendulum movement, and failed to extract the head. I sent for my friend Dr Charles Bell, who kindly came to my assistance. The forceps were again applied, and the pendulum motion, with powerful traction, was successful in delivering the patient of a fine, large, healthy boy; the perineum was torn, but not through the sphincter ani. The wound was carefully sponged, and brought together with the interrupted suture in three places in less than half an hour after the accident. The result was most satisfactory, and the patient made an excellent recovery. The ligatures came away in four days, and the wound was absolutely healed in ten, at every point. The usual rules were enforced.

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[1] Neligan's Medicines, their Uses and Mode of Administration.

Case 2.—To this one I would direct attention. On the 28th of June 1875, I was summoned to see Mrs M., æt. 35, a primipara. At 6 P.M. the os uteri was small (size of a shilling), although the patient had been in labour for twelve hours. I was again called at 6 A.M. next day, when I found the first stage almost over, and the head presenting in the occipito-anterior position. The woman had been twenty-four hours in labour, and as I considered it unjustifiable to leave her longer, I sent for the forceps. The vagina was hot, and the pains were becoming feeble. While under chloroform, I used steady traction during each pain, allowing the external parts time to dilate slowly. Notwithstanding every care, the perineum ruptured right along through the sphincter ani, and into bowel three inches, my whole index finger easily passing from bowel into vagina. When the placenta was expelled, and the uterus contracted, the wound was carefully sponged. The anæsthesia being maintained, the torn parts were brought together with the interrupted suture. Seven ligatures were used, which had been dipped in carbolic oil, and the wound was left in perfect approximation. The urine was drawn off every twelve hours. The thighs were tied together, and by the administration of opium, the bowels were confined for six days. No local dressings were used. The patient made a perfect recovery; the wound healed throughout at every point; and on the fourteenth day she was left to her own care. Several weeks ago, I examined the patient by placing one index finger in the bowel, and the other in the vagina, and found the recto-vaginal septum complete. Let me here mention, in connexion with her history, that when Mrs M. was married, I understood that perfect sexual intercourse was precluded for some months in consequence of the extreme rigidity of the vagina, and four years elapsed ere this child was born.

Remarks.—1st, Causes of laceration of the perineum; 2d, Means of prevention; 3d, General rules of treatment. Many obstetricians will agree with me in saying that, in numerous primiparous cases, the perineal portion of the vaginal mucous membrane is frequently ruptured, and only heals by leaving a sulcus, which rather favours than hinders future labours. Among the causes of perineal rupture might be enumerated, 1st, When the age exceeds *thirty* years; 2d, Cases where the head of the child is very large; 3d, Malpresentations; 4th, A small or deformed pelvis; 5th, The use of forceps; 6th, A rigid perineum; each cause operating more especially in primiparous women.

Prevention.—I generally adopt the plan of having lard, butter, or cold cream, rubbed over the perineum, during the extrusion of the head forwards. 2d, Gentle dilatation of the external parts with the finger may be adopted during each pain; 3d, Slow traction, when the forceps are used, and only during each consecutive pain; 4th, The application of the hand in supporting the perineum during strong expulsive pains; and when forceps are employed, during the delivery of the head, the left hand may be spread over the distended surface of the perineum.

Treatment.—In simple cases of laceration of the mucous membrane of the vagina, or even where the margin of the sphincter vaginæ is torn, the only treatment necessary is mere cleanliness and sponging. I extremely deprecate, in any case, the use of bandages, pads, or plasters, as being more irksome to the patient than useful. In severe perineal rupture, as in Case 2, the immediate closing of the wound is of paramount importance, so as to secure healing by the first intention. The interrupted suture of carbolized cat-gut should be used; and the entire rupture must be brought into exact approximation. Careful and frequent sponging must be attended to by the nurse, to avoid any irritation from the lochial discharge. The urine must be drawn off every twelve hours; no dressings applied; the patient kept in the horizontal position; the thighs kept together; and the bowels must not be allowed to move for six days.

ARTICLE VII.—*On Inflammation.* By G. THIN, M.D., London.

(*Continued from page* 515.)

CHAPTER III.

SPINDLE-CELLS AND COLOURLESS BLOOD-CORPUSCLES.

THE spindle-cells in the cornea are, as I have already stated, exceedingly difficult to observe in the healthy tissue. In the inflamed cornea, on the contrary, they are usually very prominent objects. This depends partly on the fact, that in inflammation their protoplasm becomes more granular, and their nuclei are more susceptible of staining.

Accordingly, whilst I am not aware of any author having described them in the healthy cornea, their existence in which has been overlooked, I have found appearances described by nearly all the authors who have lately written on the inflamed cornea, which I can without any violence apply to the inflamed spindle-cells. Some of these writers, indeed, have described them unequivocally as spindle-cells, formed during the inflammatory process by a metamorphosis of the stellate cells.

A brief reference to these notices will have a special interest for some readers, and will be useful to those who wish to study the literature of the subject for themselves.

This account is, of necessity, incomplete, and its arrangement is determined by the order in which the writings referred to happened to be accessible to me.

Norris and Stricker[1] describe the "cornea-corpuscles" of the frog after a few hours' (in den ersten Stunden) inflammation produced by cauterization with nitrate of silver as being "either flat and having several processes, many of which divide, or as oblong and spindle shaped."[1] "This last form," they remark, "is often extraordinarily prevalent, at other times it is scarce. We have met with it in the vicinity of the eschar, and at the periphery of the cornea, near the pigment. The spindle elements are sometimes single, at other times accumulated in groups, which are arranged in two directions which cross each other at a right angle."

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[1] Studien, page 3.

Again,[2] describing a cornea in which inflammation had been produced indirectly by a thread passed through the bulb and the excised cornea, treated on the second day by chloride of gold, the same observers remark, "We find on the periphery of the cornea, a part equal in extent to the whole field, in which there is nothing but spindle elements, which are arranged in two directions at right angles to each other. The protoplasm of these cells is relatively scant, and they contain invariably two or three rounded nuclei."

Kühne is cited by Stricker[3] as having seen on the border of a frog's cornea, which he had mechanically irritated, stellate corpuscles become spindle-shaped.

Recklinghausen[4] states that in the inflamed cornea he found two kinds of cells, which he did not find in the healthy cornea, namely, migrating cells, larger, and moving more slowly than similar cells which are seen in the healthy cornea, and spindle-shaped cells having fewer processes than the "fixed" cells, sometimes rounded at one end and having a process at the other. These, he suggests, may be a transition form of "fixed" cells which are becoming movable.

Hansen[5] states that in the inflamed cornea he saw "transition forms, from undoubted cornea-corpuscles to elongated perfectly spindle-shaped structures, which are to be considered as having reached the extreme degree of change of form." He also frequently found parts of the cornea where there was "not a single normal cornea-corpuscle remaining, the whole field being filled with small oblong bodies crossing each other." He suggests as a possible cause of this supposed change of form, that the "corpuscles" are acted on by a stream which, passing through the cornea, washes them into the interfibrillary spaces, whose arrangement in parallel and crossing layers they accordingly assume.

Böttcher,[6] instead of nitrate of silver, used chloride of zinc, and when he cauterized a limited point in the centre of a frog's cornea by this irritant, he succeeded in producing inflammatory changes, which were independent of any change that proceeded from the conjunctival vessels next the corneal border. As part of the process by which these changes were produced, he believes that in three days all the stellate cells had been transformed into spindle cells.

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[1] The term "spindle-cells" here, and already so often used, is equivalent to the "fusiform cell" of many English authors.

[2] Loc. cit., p. 11.

[3] Handbuch, p. 14.

[4] Eiterung, etc., Virchow's Archiv, Bd. 28, p. 180.

[5] Wien. Med. Jahrb., 1871.

[6] Virchow's Archiv.

We shall see further on, that the same structures described by the above-named authors as spindle-cells derived from the "cornea-corpuscles," have been seen and described by Cohnheim and others as colourless corpuscles in spindle form.

Both schools agree in believing that the spindle-shaped structures arise, *de novo*, as a result of inflammation, but the one believes that they are transformed, pre-existing elements; the other, that they are peculiarly-shaped, colourless blood-corpuscles, which have entered the cornea from the nearest bloodvessels.

The strictures of the one of these schools on the views of the other are in both instances so well-founded, that it seems to me that the controversy must last as long as the views which have been hitherto held regarding the structure of the cornea hold their ground. As these have already lasted long enough to establish a kind of prescriptive right to a place in text-books and prelections, they will probably die hard, and no one can tell how many successive generations of students may yet hopelessly struggle with "spindle-shaped bodies," "dagger-like figures," and "negative and positive images."

I have previously shown that it is possible to see in the healthy cornea, chains of minute spindle-cells with fine connecting processes. These correspond in number, in arrangement in parallel rows, in the fact that the rows in one plane cross those in another, and in the relative distance of the nuclei from each other, with the spindle-shaped bodies seen in the inflamed cornea. The presumption, therefore, that they are the same is so strong, that almost the only proof required is that of the existence of spindle elements in the healthy tissue. To demonstrate them in health, requires, as we have seen, the employment of special methods.

In the inflamed cornea, they can be seen by several different methods. Those chiefly employed during late years, have consisted in examining the freshly excised cornea in aqueous humour, and in staining with gold. Norris and Stricker, for example, have described spindle-cells seen in a cornea, examined in aqueous humour after a few hours' cauterization. I have found, however, that they can be seen more distinctly, and their relation to the other elements of the cornea better understood, if the excised cornea is sealed in aqueous humour or blood-serum, and examined after 24 to 48 hours.

My best preparations were obtained in the following manner:—Choosing a large frog (*Rana esc.*), I touched the centre of the cornea with a fine point of nitrate of silver, and immediately neutralized the caustic by placing a drop of half-per-cent. salt solution on the eye. In winter, all inflammatory changes in the frog proceed more slowly than in summer, and in the esculent frog, more slowly, I think, than in the common English frog (*Rana temp.*). I therefore made a series of experiments in winter on the large Continental variety. The conditions in which I happened to be placed, made it inconvenient for me to examine the cornea at other than invervals of twelve hours after cauterization, which is the only reason why the first changes I describe are those seen after the first twelve hours. I have also, at other times, examined frog corneæ after various much shorter intervals, but I have found nothing which makes it necessary to describe them further.

In a cornea treated as I have above described, sealed in serum, and examined after twelve hours, the following points may be observed if the preparation has been a successful one. Adjacent to a part in which the corneal epithelium is wanting, there may often be seen a part in which the contours of an epithelial layer are marked by the ordinary dark network generally observed in silver preparations. The epithelial cells have been affected by the silver, and are much more easily seen than the epithelium on other parts of the cornea. In the part which has been denuded of its epithelium, there can be detected, by careful examination with a magnifying power of 300 or 400 diameters, oblong, shuttle-shaped bodies, arranged in rows, and maintaining generally a somewhat constant distance from each other. Their greatest breadth approaches roughly that of a human red blood-corpuscle, and their length is about a half more. They are composed of a colourless, very finely granular substance, the appearance of which justifies the application to it of the general term protoplasm. A more careful examination enables us to detect in the protoplasmic mass one or more elongated nuclear bodies, and at either end a fine process, which is continued into a similar adjoining protoplasmic body. Each mass, consisting of a nucleus (or nuclei), protoplasmic substance, and terminal processes, constitutes the histological element known as a spindle (or fusiform) cell. The protoplasm, scant in quantity at the centre of the cell, is continued for a very short distance into the process. The processes are seen as fine cylindrical thread-like structures, whose refractive properties differ considerably from those of the cell-substance. The arrangement of the cells is in rows or chains, parallel to the direction of the bundles of fibrillary or corneal substance, and to each other.

Fig. 27 represents part of a preparation, such as those which have formed the basis of this description. The epithelium is seen slightly acted on by the nitrate of silver, and where it is wanting are seen the chains of spindle-cells and processes above described. The figure gives a good idea of the relative size and position of the cells and processes, but it is impossible in a woodcut to represent accurately the peculiar refractive properties which characterize them. In successful preparations, their individuality is more evident than it is possible to show in an engraving.

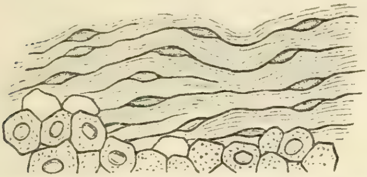


FIG. 27.—Part of a frog's cornea (*Rana esc.*) twelve hours after being touched by a fine point of nitrate of silver. Sealed serum preparation. Drawn after the cornea was forty-eight hours sealed. The spindle-cells are at one level and are parallel to the transverse plane of the cornea.—Magn. 400 diam.

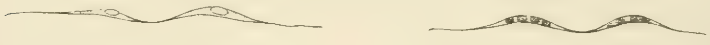
It can sometimes be seen that the chains of spindle-cells are visible in a corneal tract, while they are not seen at all at the other parts of the structure. In such cases, the visible spindle-cells can be traced to the border of the cauterized spot, and the area in which they are visible is bounded by one of the walls of a cleft. On the other side of the cleft, there may be no evidence of their existence. The tract in which they are seen is such as has been shown in Fig. 20. The preparation from which the drawing there represented was made, if it had been sealed in aqueous humour, instead of having been treated by gold solution, would probably have shown in the inflamed tract, instead of the dark areas of metallic deposit, chains of spindle-cells similar to those represented in Fig. 27.

The fact which this method teaches us is, that in inflammation the spindle-cells and processes have undergone a change, causing their refractive properties to differ from those of the tissues amongst which they are situated.

The inferences derived from sealed preparations acquire certainty by comparison with osmic-acid preparations, in which we more particularly get accurate notions regarding the increase of the protoplasm in the cells.

I have used this reagent for this purpose principally in the cornea of the rabbit. A thread was passed through a portion of the cornea, and the animals killed at periods of one, seven, twelve, and fifteen days. The cornea was then excised, and placed in a half-per-cent. solution of osmic acid for twenty-four hours. Sections can then be made without further preparation, but as it is desirable to have as many sections for examination as possible, I have usually divided the cornea into several pieces, which I imbedded in wax and oil, to facilitate cutting.

In a cornea which has been inflamed for only twenty-four hours, sections can be obtained in which the even homogeneous ground substance is broken by elongated, spindle-shaped, protoplasmic masses, widest at the centre, and tapering to a point at either extremity. The protoplasm, stained by the osmic acid more darkly than the ground substance, is coarsely granular in such sections. In the thinnest sections, it is possible to trace a very fine fibre connecting the contiguous extremities of the spindle-shaped masses. If the section is stained in logwood, two to four—generally four—small nuclei are seen in the centre of the protoplasm. The arrangement of these small nuclei is invariably in a row. They are never grouped in a cluster, and in this respect they differ from the nuclei of the white blood-corpuscles. As an important point in making the diagnosis between spindle-cells and other structures with which they may be confounded, I lay great stress on this linear arrangement of the divided nuclei.



(a) (b)

FIG. 28.—Spindle-cells from inflamed cornea of a rabbit. Osmic acid. Logwood.

a) Increase of cellular protoplasm. Nuclei entire. (b) Nuclei divided. Magn. 400 diameters.

In sections of a rabbit's cornea, which had been twelve days inflamed, I found in the position of the spindle-cells, near the corneal border, at a part removed from the chief seat of inflammatory change, long protoplasmic columns, presenting slight constrictions, and containing nuclei in their interior. The distance of the nuclei from each other was similar to that which is the rule for the spindle-cells. Of the connected and individual character of those protoplasmic columns, I was able to satisfy myself in teased preparations. Part of one of these columns is represented in Fig. 29. From the position of the columns relative to the bundles of corneal tissue, the position and size of the nuclei, and from the absence of any other indication of spindle-cells, I infer that it is with spindle-cells we have here to do. But whether the condition of long protoplasmic columns is due to inflammatory change in the original spindle-cells, or whether we have before us a new formation—being part of a tissue destined to repair the injury which has been sustained—I am unable to say.

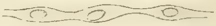


FIG. 29.—From inflamed cornea of a rabbit Osmic acid. Red aniline. Dilute acetic acid. Magn. 400 diam.

The aniline dye, sold in England as *red aniline*, gives most instructive preparations of the spindle-cells when they have been previously fixed by osmic acid. The protoplasm of the cells is stained a deeper tinge than the surrounding tissues, and becomes prominently visible. If the section is stained very deeply in the aniline, and a drop of acetic acid is drawn by blotting paper through the glycerine in which it is mounted, and the effect watched under the microscope, the aniline colour is seen to disappear rapidly. There is a particular stage in this rapid bleaching process in which the contours of the spindle-cells are well marked in relation to the cornea substance in which they lie. At the same time, the contours of the nuclei are well seen. Permanent preparations of this kind are difficult to obtain, because it is not easy to control the bleaching effect of the acetic acid. Very beautiful preparations can be got by a double staining with red aniline and logwood.

The number of spindle-cells seen in such preparations varies according to the intensity and extent of the inflammatory process, and according to the extent to which the plane of the section happens to correspond to an inflamed tract.

The knowledge obtained by examining sealed serum preparations of the frog's cornea is thus confirmed by the osmic-acid preparations of the rabbit's cornea, and not only confirmed but extended.

The protoplasmic nature of the spindle-shaped body is made manifest by the granular, definitely-contoured substance, which is made prominent by the osmic acid. The nuclei are so well seen that the knowledge regarding them becomes exact. We find that the nucleus of each cell has divided into several distinct, small, rounded bodies, placed end to end. These readily stain by logwood and red aniline, which the normal undivided nucleus does not, from which it is to be inferred that the nuclei have not only divided, but have undergone a chemical change, by which their composition approaches that of the nuclei of the cells ordinarily seen in connective tissues, which are always readily stained by logwood.

When it is said that the nucleus divides, it is necessary to revert to the evidence obtainable regarding the condition of the nucleus in its normal condition. When these cells are demonstrated by gold solution being injected from the arterial system, the nucleus is usually seen as a single oblong body. Not unfrequently, however, the position of the nucleus is occupied by two oblong bodies, placed end to end—an appearance which seems to indicate the condition in which a double nucleus exists in these cells. As four small nuclei are often observed in the inflamed cell, this would, in the case of a double nucleus, represent division of each of them into two parts.

In the healthy structure, the nucleus of the spindle-cells can be occasionally seen in a frog's cornea, examined in half-per-cent. salt solution, and it is then usually observed as a single oblong body.

We have thus obtained evidence that in an early stage of inflammation of the rabbit's cornea, before colourless blood-corpuscles are found in the sections (and when they are present, they can always be seen), the spindle-cells enlarge by an increase in the cellular protoplasm, and the nuclei divide.

In osmic-acid preparations of a rabbit's cornea which has been inflamed a week, we find at many parts of the cornea enlarged spindle-cells, similar to those I have just described as being seen in the earlier stage. But here we are able to compare them with the stellate cells and colourless blood-corpuscles. Their relation to the colourless blood-corpuscles will be better understood after I have described the latter. Their relation to the stellate cells is the following:—In a large proportion of the osmic-acid preparations, in which the spindle-cells and colourless blood-corpuscles are very conspicuous objects, nothing of the stellate cells is visible, whilst in a small proportion of such sections they become at once visible. In the very thin sections they can, however, usually be seen. In many sections in which they are not previously visible, they are seen after staining by the solution of red aniline, which is the method I recommend for their demonstration. In sections in which they are not previously visible, solution of extract of logwood and alum sometimes demonstrates them by a uniform staining of the cell substance; but it is somewhat difficult to manage this process, except in extremely thin sections, as the ground substance is apt to stain too deeply to allow the cell to become visible. The aniline staining is for this purpose much to be preferred.

The stellate cells and their fine processes are seen in most cases unaltered, even when they are surrounded by rows of swollen spindle-cells, and great numbers of colourless blood-corpuscles. In some parts, the globular form of the cell, and the beaded appearance of the processes previously described, are seen. Of the condition of the nucleus of the stellate cells in such preparations, little information is obtainable. When the osmic acid has acted alone, and no additional dye is used, the nucleus is invisible. When there has been subsequent staining by logwood and aniline, the stained nucleus can be seen in the body of the cell, but not with sufficient distinctness to permit an accurate judgment regarding its condition. It is noteworthy, that in preparations in which the contours of the nuclei of the spindle-cells and white blood-corpuscles are indicated with the greatest precision by logwood and aniline, the nuclei of the stellate cells, although stained, are still obscure.

(*Chapter III. will be continued in our next Number.*)

ARTICLE VIII.—*Clinical Lecture on Intermittency and Irregularity of the Pulse, and on Palpitation, Cardiac and Aortic.* By GEORGE W. BALFOUR, M.D., F.R.C.P. Ed.

GENTLEMEN,—There is perhaps no single symptom connected with the heart which gives rise to so much discomfort, uneasiness, and feeling of insecurity, as intermittency or irregularity of the pulse; yet both of these phenomena are perhaps at least as frequently associated with the absence of serious disease of the heart as the reverse, and this is especially the case with intermittent action. Nevertheless, intermittent action of the heart is so alarming in itself, and so frequently occurs in those who are otherwise of a highly nervous temperament, that it not unfrequently constitutes a very serious complication, almost worthy of being regarded as in itself a disease.

Simple intermittence is the slightest form of derangement of the cardiac action, and consists in the occasional omission of a pulsation, the next recurring at the usual period, without any alteration of the cardiac rhythm. The intermission may occur once every two beats, or once every twenty, forty, or more; it may consist in the omission of only one pulsation, or two or three pulsations may be omitted each time, or one pulsation may be generally omitted, and at occasional times we may have the intermission extending over two or three pulsations, thus introducing the element of irregularity in its simplest form. In these cases we have the pulsations equal in strength, but with intervals of varying length between them. But this irregularity is frequently not confined to the intervals, but extends to the pulsations themselves, which in such cases not only occur after pauses of uncertain and varying length, but are themselves of very unequal strength. In the simplest form of irregularity we recognise the truthfulness of Dr Richardson's description. He says, "The ventricle continues in diastole for two or more strokes of the systole of its auricle, and then relieves itself by a prolonged effort: it is like a man who, striking at the forge a number of strokes in rhythmical succession until tired of the action, changes it for a moment to give a more deliberate and determinate blow, and then rings on again in regular time."[1] But irregular pulsation of the kind last referred to does not come under this description. Of it we may have a variety of forms, which may be referred to two heads: in the first we have the hammer ringing on in regular time upon the anvil, but only striking the irregular blows at irregular intervals; in the second we have the irregular blows made still more strikingly irregular by occasional cessation of the regular ringing which carried on the continuity of the action: while we may also have the effective blows, the ineffective ringing, and the total cessation of action combined in every possible variety of rhythm. Thus a heart, which usually or occasionally intermits, may at long, and uncertain intervals of time, have suddenly, without any known provocation, a series of half-a-dozen or so of rapid ineffective ventricular contractions, usually felt as a sort of fluttering by the patient himself,—a sensation which the practitioner also recognises when he has the chance to feel them either in the radial artery—which they do not always reach—or in the cardiac area itself, and these flutterings once passed may not recur for months. At other times the pulse and the cardiac action itself present such a combination of irregular intermissions, unequal pulsations, and occasional thumpings, as can be likened to nothing else than an irregular game of hop, step, and jump, in which each element is liable to be omitted, to be repeated more than once with varying and unequal effect, and also to be transposed in every possible way, till you actually think that the heart must have forgotten how to beat and must soon cease; yet all this may go on for hours, days, months, or years, I do not say without detriment, but I can safely say without any apparent detriment to the patient's well-being and even usefulness in life, provided he be not called upon for any very active, or at all events long-continued, exertion. At first such irregularity is no doubt very alarming, but it never becomes extreme at once, usually beginning by occasional intermissions and gradually passing into its severer forms, though occasionally at any period of life, but especially about middle life, one may be suddenly surprised by an attack of extreme irregularity without previous warning; but in such cases, as a rule, it soon passes off, seldom lasting more than a few hours. It is only in advanced life that we find irregular action persisting continuously for months or years.

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[1] Transactions of the St Andrews Medical Graduates Association for 1869, p. 234.

Palpitation is merely the occurrence of abnormally rapid pulsation in the heart or in the aorta, most usually the abdominal aorta. This may persist for only a few minutes and then die off, or it may last continuously for a much longer period.

All these abnormalities of cardiac action are very closely connected together, and they are all neurotic in character. That is to say, the nervous system plays a most important part in their production. I do not say in their origination, because, though that is sometimes the case, it is by no means always so.

We know that the heart of an embryo chicken has been seen to beat (Funke, Lebert, etc.) when merely composed of cells, long before any nervous structures were visible—while, therefore, the nervous force to which its action was doubtless due was nothing more than a diffuse *vis insita.* By-and-by, however, nervous ganglia are formed in the substance of the heart, and to the influence supplied by these ganglia most modern physiologists seem inclined to attribute the ordinary rhythmical movements of the heart. It is true enough that in the adult heart we cannot disconnect these movements from the ganglia, because to remove the latter we would require so to dismember the heart as to destroy its capacity for rhythmical movement, or for any continuous movement whatever. But, inasmuch as these rhythmical movements commenced and continued long before the development of any ganglia, it seems reasonable enough to conclude that the same *vis insita* still maintains them after the formation of these ganglia, and would maintain them after their removal, if that could be done without destroying its perfection as a piece of mechanism, and that it is to this *vis insita*, and not to the retention of these ganglia, that the rhythmical movements of a shark's or a frog's heart are due,—movements which may last for hours after the complete severance of the hearts from the respective bodies of the animals to which they belonged. From this point of view it would seem that these ganglia are developed, not so much for the purpose of providing a nervous force, which already exists and thoroughly pervades the cardiac muscle, as rather for that of connecting the heart with the rest of the organic frame, and co-ordinating the cardiac movements with the multifarious requirements of that frame, both in health and in disease.[1] But for this co-ordination the heart would, to the great and manifest inconvenience of the organism, maintain one uniform rate of pulsation in the stillness of repose, and amid the turmoil of the most strenuous exertion; and with an impartiality even more remarkable than that of death itself, would march to its final repose at the same steady and unfaltering rate through the depression of cerebral coma on the one hand, or the excitement of violent fever on the other. No doubt, for wise, if not always for very obvious purposes, these cardiac ganglia have been developed, which connect the heart and its movements on the one hand with the sympathetic nervous system, mainly through the lower cervical ganglion, and on the other hand with the cerebro-spinal system, chiefly by means of the inferior cardiac branches of the vagi. The term *excito-motor* has been applied to the one system, the appellation *inhibitory* to the other, and I leave these terms as they stand, doubting if we have as yet sufficient information as to the actual and reciprocal action of these two systems on the heart to warrant their acceptance in the ordinary meaning with which they are employed. The sympathetic nervous system we know presides over ordinary nutrition, maintains the bloodvessels in their normal condition, and, when excited, constricts them; while the cerebro-spinal system has for its chief action the maintenance of the general *consensus* of the organism, and by propagating influences from one organ to another, antagonizes the sympathetic and permits dilatation of the arterioles and the passage of blood in quantity sufficient for rapid and increased secretion, etc. But the heart is not an arteriole, and the difficulties in the way of understanding the actual influence of these two systems upon the heart is not lessened when we find that Bernard states that section of the vagus in the neck lessens the respirations by one half, and doubles the cardiac pulsations, while Brown-Sequard alleges that in the frog at least, section of the vagus has no influence whatever either upon respiration or cardiac pulsation. Be that as it may, however, it is enough for us for the present clearly to understand that the heart beats rhythmically by virtue of a *vis insita*, and that its pulsations may be variously modified by influences conveyed to it through the two great antagonistic nervous systems—the sympathetic and the cerebro-spinal; and, till physiology comes to the rescue with some more definite and more universally received views than any she has yet produced in this matter, we must content ourselves by feeling along the lines of nervous communication in any case of palpitation, intermittence, or irregular action, for any perverted action or secretion which may be capable of affecting the heart reflexly ; and in doing so we must employ our reason as well as our observation ; and we must also carefully determine whether there is anything in the mechanism of the heart itself which renders it more than usually susceptible to extraneous injurious influences.

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[1] I am glad to learn that this important doctrine, which I have always held and taught—*vide* Introduction to the Study of Medicine, Edin. 1865, p. 97—is now being recognised by physiologists. *Vide* Proceedings of the Royal Society, vol. xxiii., No. 160, p. 318, and Ed. Med. Journal, Oct. 1875, p. 370.

It is somewhat remarkable, that so long as the heart remains mainly under the influence of the *vis insita*, and unaffected by extraneous agencies acting through its nervous connexions—that is, so long as the fœtus remains *in utero*—intermittent action is unknown. It must occasionally occur, because, even *in utero*, agencies are now and then at work which ought to produce it. But its occurrence must be very exceptional, as I know of no such case recorded, and have inquired at several friends, much in the habit of auscultating fœtal hearts, without ascertaining that intermittent action has ever been observed in such cases. But no sooner is the child born than injurious agencies multiply and increase in intensity, and by their reflex action it comes to pass that we have intermittent cardiac action at once developed in so many instances as to constitute infantile intermittence a well-known fact. And from man's birth to his grave it is a phenomenon of frequent occurrence, though in itself it has as little influence in precipitating his exit from this world as in influencing his entrance on its stage. The late Dr Matthew Baillie was once consulted by a patient with an extremely irregular pulse, and being pressed to give a reason for its occurrence, he replied, "I don't know what is its cause, but I can tell you this, it won't shorten your days." The patient, a native of this city, was forced to content himself with this opinion, and lived to prove its truth, for he died at a very advanced age many years subsequently. But it is not always so innocuous ; for while it always indicates some weakness of the cardiac walls, or some irritability of cardiac innervation, not always of much importance, so it many times is also associated with serious lesion of the valves, or of the cardiac muscle, which does tend to shorten life.

I have already mentioned[1] to you that extremely irregular action is almost pathognomonic of mitral stenosis ; Mr Adams of Dublin[2] regarded it as quite pathognomonic of this lesion, provided the irregularity were of such a character that several of the faint fluttering cardiac pulsations every now and then failed to be felt at the wrist. In this he was no doubt wrong, as irregularity of this character is at least as common in the gouty heart, whether dilated or not, as in mitral stenosis ; and he was also wrong in following Corvisart and attributing extreme irregularity in both force and frequency of pulse to narrowing of the aortic orifice. Elliotson corrects this opinion, for he says, "I do not happen to recollect a pulse irregular in force and frequency in a single case of narrowing of the aortic opening solely, while I know that it is very common in the narrowing of the left auriculo-ventricular opening, though possibly not peculiar to it, nor indeed to narrowing of any opening."[1] My own experience leads me to coincide with Elliotson's opinion, which I believe will be found to be correct. To constitute it pathognomonic of mitral stenosis, irregular cardiac action need not present any special form of irregularity, for that varies in each case, but it must be associated with some at least of the other concomitant phenomena diagnostic of this lesion, and most certainly with accentuation of the pulmonary second sound, a phenomenon not always easily recognised in such cases, because both second sounds are apt to be much weakened when the cardiac action is extremely irregular; still in mitral stenosis it will be always possible to discriminate the pulmonary second as a shade more distinct than the aortic second, though it may not be markedly accentuated. Very frequently in such cases the existence of an irregular mitral systolic murmur simplifies the diagnosis, though this is by no means always the case. In all such cases there is also evidence of cardiac failure from pyrexia or overwork, frequently a history of rheumatism, and very often well-marked signs of pulmonary and of general venous congestion. I know of no cardiac cases more amenable to proper treatment than these are, none in which relief is so certain of attainment, though of course perfect cure is unattainable, and even complete removal of the irregularity is hardly to be expected, though it is sometimes temporarily obtained. In all such cases the pulse is of course permanently small and feeble even when it is not irregular, and this—as well as the presence of pulmonary accentuation—constitutes an important point in the differential diagnosis between irregularity depending on mitral stenosis, and that depending simply upon gouty weakness of the cardiac muscle. In the latter class of cases, with which for diagnostic purposes we may associate atheroma of the coronary arteries, also said to be an occasional cause of cardiac irregularity, we have of course no accentuation of the pulmonary second; in the intervals between the fits of irregularity the pulse is of the normal fulness and strength, and this is also the case with those pulsations which during the attack fairly reach the radial artery. In mitral stenosis then, while some of the cardiac pulsations fail entirely to reach the radial artery, even those which do reach it are all small and feeble; but in the gouty heart, while some of the pulsations also fail to reach the radial artery, most of those which do reach the periphery are of average strength and fulness. The chief exception to this rule is when the gouty heart is already dilated and the mitral valve incompetent, but in these cases we have superadded to the irregular action, all the ordinary signs of dilated hypertrophy to guide our opinion. Once fairly developed there is probably no class of cases, not associated with absolute organic disease, in which a permanent cure is so rarely attained as in gouty irregularity, though they are susceptible of great relief from appropriate treatment, and with moderate care are not usually attended by any considerable shortening of the normal span of life; some indeed with even extremely irregular gouty hearts continue in fair health to a very advanced age, without any obvious impairment either of bodily or mental vigour, except that depending upon senescence.

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[1] Ed. Med. Journal for Jan. 1872, p. 616.

[2] Dublin Hospital Reports, vol. iv. p. 494.

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[1] On Various Diseases of the Heart, fol. London, 1830, pp. 16, 17.

There is one form of abnormality, however, in a gouty heart which I do not hold to be so susceptible of a favourable prognosis. I refer to that to which attention is ordinarily first directed by the occurrence of symptoms referrible to cerebral anæmia, in which abnormal slowness of the pulse is the most remarkable symptom—a slowness of pulse not because the cardiac action is slow, but because many of the pulsations fail to reach the periphery. It is long since Hope pointed out that though cardiac action was occasionally really slow, yet the bulk of these cases in which the pulse was said to range from 20 to 30 a minute, were really cases in which one or two of the ventricular beats were regularly and permanently imperceptible in the pulse, every second or third pulsation only reaching the periphery;[1] and Dr Stokes has made some most important and instructive remarks on the same subject, expressly in connexion with the production of cerebral anæmia, and all the peculiar and important phenomena which may result from that.[2] In one very instructive case which I saw some years ago, an old lady, long gouty, though without regular attacks, and with a dilated feeble heart, had been suddenly seized while shopping with what seemed to be an epileptic attack. These attacks continued to recur when she made the slightest exertion, and at the time I saw her she was unable to rise from the recumbent posture without bringing on one. They were associated with flatulent dyspepsia and an extremely slow pulse, averaging about 20 per minute. On careful examination I found that her heart acted with perfect regularity, but with unequal force, so that the apparently abnormal slowness of the pulse was simply due to the fact that only about every third beat reached the periphery. It was not difficult to connect this defective cardiac action with diminished arterial pressure, perfectly sufficient to produce all the phenomena of cerebral anæmia on the one hand, and among these epileptic seizures, and also defective secretion generally, and as one of the results of that flatulent dyspepsia. There is but one remedy that I know of capable of remedying this state of matters, if, indeed, the muscular fibre be still perfect enough in structure to permit its being remedied, and that is digitalis; full doses of that were prescribed, and with nutritious diet and a fair allowance of stimulants, it succeeded so well in her case, that within a week she was going about as usual, was so well indeed, as to give a dinner party, and she died gradually from asthenia some years subsequently, without any return of these serious symptoms. Such cases are, however, always more serious than those of simple irregularity of the cardiac action, and are not always so easily to be remedied, considerable degeneration of the cardiac muscle being only too frequently concomitant.

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[1] On Diseases of the Heart. London, 1839, p. 337.

[2] The Diseases of the Heart and the Aorta. Dublin, 1854, p. 362, etc.

Irregularity of the pulse is, you will see, most frequently associated with mitral stenosis or with gout; no doubt it is also associated with other cardiac affections, or with apparently simple dyspepsia, rarely, however, I think, unless these affections or that dyspepsia be associated with the goŭty dyscrasia. Simple intermittence, however, stands in a somewhat different category; sometimes it is only an early indication of failure of cardiac power dependent upon anæmia, over-work, or worry, or upon valvular disease or gout, but it is often a purely nervous phenomenon. In the former class of cases we have the affection commencing by an intermission, followed by a thump. As the disease progresses this thump becomes associated with a sensation of tumbling, and by-and-by the irregularity and inequality of the heart's action reveals itself to the sufferer by a rapid and irregular succession of thumps and tumbles of varying force. In the class of cases, however, to which I now refer, the disease never progresses beyond the thumping stage, and the thumps are not even very distinct; the patient has at the most an uncomfortable sensation in his cardiac region of varying intensity, lasting for less than a second, and if we happen to feel the pulse at that moment we become aware that this uncomfortable sensation is associated with the omission of a radial pulsation and nothing more; and sometimes this takes place regularly without the patient being in any respect conscious of it. Such cases are of purely nervous origin, and arise from sudden fright, grief, or anxiety; and the intermission thus produced, though for a time of frequent recurrence, gradually dies out, and sometimes disappears entirely, while in many cases it remains permanent, though much more infrequent than at first. This form of intermittence originates in the accidental coincidence of fright or anxiety with a heart congenitally or acquiredly feeble, or with a nervous system from similar causes unduly impressionable. I have known it commence with the shock of a railway accident, and in that case the intermissions were at first every second beat, but in a few months came down to one intermission in twenty beats, and I have no doubt will ultimately disappear. Richardson has known it follow shipwreck in one instance, with a somewhat similar result, and sudden grief, anxiety, or anger, in several other cases.

This form of intermission is simply an exaggeration of what we have all probably felt at one time or other when our heart has, for the moment, stood still in the face of any impending danger to ourselves or others. It is merely an aggravated form of what our vernacular poet James Smith has so graphically described as indicative of maternal anxiety:—

> "My vera heart gaes loup, loup,
> Fifty times a day."

This "loup" being nothing but the perceptible thump which succeeds a momentary intermission. In nervous, and therefore impressionable individuals, now and then, there is no returning thump, the intermission, as Paddy would say, becomes permanent, and we have sudden death from emotional causes, of which not a few are upon record. At other times, partly from the intensity of the impression, but chiefly from debility of the nervous system, this "loup," thump, or intermission, of which the loup is the most striking subjective symptom, not only occurs under the instantaneous excitement of any emotion, but repeats itself, at first at shorter, afterwards at longer intervals, until at length it dies out under the reassertion of the normal condition of the nervous system. Now and then, however, it never dies out, but repeats itself so long as life continues. We may, however, be permitted to doubt in such cases if the emotional excitement have any other connexion with the intermittence except simply as the accidental incentive to a series of actions already from other causes about to begin. Be that as it may, however, the connexion between emotional excitement and muscular motility of a convulsive and rhythmical character is not an unknown thing in other departments of medicine; and cardiac intermittence and irregular action from emotional causes, finds its counterpart in those imitative choreas and epilepsies, which are of no infrequent occurrence, and has even no very distant connexion with the vagaries of the convulsionnaires and choreomaniacs of the Middle Ages.

In the treatment of intermittent or irregular pulse, we must be guided very much by the condition of the patient, and the existence of any actual cardiac disease, or of any irritation capable of reflexly producing such irregularities. And this we must carefully ascertain for ourselves, and never trust to the mere statements of the patient; because there is nothing more common than for a dyspeptic patient to say, "I never have a headache; I may eat and drink what I please, my stomach never troubles me." True, but his heart does; and careful examination will discover that his stomach is not so perfect as he represents it to be. It is precisely the same with him as with a patient with neuralgia of the shoulder-joint and a decayed molar tooth. "You need not look there," he says; "I never have toothache." But he winces when we touch the tooth, and if we get leave to extract it his neuralgia is cured. The one man has toothache in his shoulder, the other dyspepsia in his heart. The cases are analogous, and teach us to put more faith in our own careful examination than in the statements of any patient, which, let me add, however, there is no need to contradict. We are bound to cure our patient if we can, but it would be both thankless and dangerous to attempt to confute all his prejudices.

In the intermittent pulse of infancy and childhood little treatment is required; the bowels must be regulated if necessary, but more by food and exercise than by medicine, for whatever enfeebles the frame tends to keep up the intermittence. We must also by moderate exercise in the open air, early hours, plenty of sleep, and the use of a nutritious but unstimulating diet, seek to tone down any nervous instability, and to develop a state of rude unconscious health. The patient ought also to be warmly clad, and the use of quite cold water as a bath avoided, as any nervous shock ought to be most carefully shunned. If any remedies seem needful, the bromide of iron is a very useful one, or in very irritable patients it may be necessary to have recourse to the bromide of potassium, for a time at least.

In patients affected with cardiac disease, we of course regard the irregular pulse as a mere symptom, and treat the central lesion upon which it depends, whatever that may be. In by far the larger proportion of cases it will be found to be mitral stenosis, as I have already told you; and as the irregular action accompanying this lesion is merely a sign of cardiac debility, what we require to do in these cases is simply to slow and steady the heart's action, increasing at the same time the force of its muscular contractions. All this we can do most effectually by the judicious use of digitalis, so much so, that in a few days the patient will express himself as feeling a new man, and he will not much mind any little remains of irregularity, which it may be difficult if not impossible altogether to remove. Of course, though digitalis in small, repeated, tonic doses, must be our main stand-by in these cases, other drugs as subsidiary agents are frequently of great importance and must be used *pro re nata;* such as carbonate of ammonia when bronchitic rhonchi are present; squill if there be much œdema—if only a little we may safely trust its removal to the digitalis; arsenic if there be much cardiac pain, which there seldom is in these cases; bromide of potassium, with or without morphia—subcutaneously or otherwise—if there be much nervous restlessness; and iron in some form or other if anæmia be a prevalent symptom.

When, however, intermittent or irregular cardiac action comes to be a subject for medical treatment apart from cardiac disease, the heart may still be soothed and steadied by small doses of digitalis if necessary, but the treatment falls mainly under three heads—*First*, to remove the cause if possible; *second*, to brace up the whole organism; and, *third*, to soothe the nervous system. If the cause be mental, such as grief or anxiety, all our care will frequently be baffled, and our success will usually depend not so much upon our remedies as upon our influence, and also upon the amount of mental firmness originally possessed by our patient, and whether he is still capable of being roused to exertion. In nervous shocks from anger or fright, we have a potent help in the narcotic needle, which timeously employed abbreviates the period of shock, and lessens its subsequent influence, besides giving the patient confidence in our resources and in their power to relieve him; the dose of morphia injected must, however, be a full one, and such as is sufficient to ensure sound sleep for some hours. We must in every case caution the patient against all depressing agencies, such as excess in venereal pleasures, excess in tobacco-smoking, too much work, especially intellectual work, all worry or excitement of any kind, and we must prescribe abundance of sleep, fresh country air, plenty of sunlight, perfect quiet, light amusing occupation, and nutritious diet in small quantities at regular intervals, suited to the requirements and capacities of our patient. But no solid food should be given at a less interval than four hours, so as to avoid introducing fresh food into a stomach still containing undigested material, as nothing is more injurious; but a tumblerful of hot water, or a large teacupful of hot solution of Liebig's extract of beef, washes out the stomach, prevents the accumulation of flatulence, and often proves most useful in stimulating the completion of digestion and the emptying of the stomach.

Alcohol is the one domestic remedy which exercises the most potent influence upon an irregular and intermittent heart;[1] it is, however, one which must be used with caution, because excess in its use is apt to perpetuate and increase the very evil it is employed to cure. Still, moderately employed, its action is not only palliative, but to a certain extent curative; only it must be employed in moderate doses, and in those forms which contain fewest substances likely to disagree, and these are in the main good sound claret, and pure whisky free from fusel oil or all injurious impurity. Next to these comes sound sherry, neither too dry nor the reverse, but of medium quality. Porter, ale, and beer are useful enough at times, as well as all other alcoholic fluids, but as a rule are not to be commended, though in every case we must be guided by the idiosyncrasy, the purse, and the convenience of our patient. The object we seek to attain is to provide a nutritive, diffusible stimulant, slightly narcotic or sedative in its qualities, and one the components of which shall disturb digestion as little as possible, while we also take care that the quantity introduced shall not be sufficient materially to interfere in this way. About two ounces of absolute alcohol is the most that can be introduced into the system in one day without detriment, but this may be given in divided doses, and in various forms, according to the requirements of the patient. Coffee, but especially tea, are excessively injurious to such neurotic patients; they ought therefore to be avoided, and a French breakfast, with meat, fruit, and claret, will be found to suit such a patient much better than our ordinary one with tea for its principal beverage; but if preferred, hot soup with a little well-boiled rice will answer equally well in most cases. For lunch, a tumblerful of milk and Carrara water, a glass of beer, or a basin of soup, according to taste. For dinner, plain roast or chop, vegetables in moderation, no pudding, and two or three glasses of claret, or a couple of glasses of sherry, will be found very serviceable. No tea, and at night a glass of whisky and potass water, with or without a biscuit. This is a sort of model diet for such cases, which must be varied to suit each individual case. We must, especially in this climate, secure that whatever is taken in the morning be sufficiently stimulating to enable the patient to withstand the cold—especially in the winter—and to soothe him under the unavoidable worries of life. To this end, if soup be preferred for breakfast, a glass of sherry, or a tablespoonful of brandy stirred into the white of an egg previously dissolved in a little water with a pinch of sugar, will be found a most agreeable and useful lunch. What we must avoid is the production of a catarrhal condition of stomach, or its keeping up, if it already exist, as to that, more perhaps than to anything else, is due the persistence of intermittent cardiac action. But in these cases alcohol is really a remedial agent from which we can obtain more good than from any other drug, only it must be used as a drug, with caution. Definite rules only apply to definite cases, but there are three grand rules which apply to every case, and these are, that the alcohol must be given in a digestible as well as stimulating form, in divided doses throughout the day, and never in excess, otherwise we shall increase the evil we desire to cure.

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[1] In this I may seem to follow Dr Richardson, but the fact was well known to me long before the publication of any of his papers.

What we desire to do in such cases is to brace up the general system, at the same time protecting it from injurious influences. Warm clothing, therefore, is a necessity, and cold sponging of the chest, especially every morning; but bathing, particularly sea-bathing, must be shunned as dangerous, the shock being only too apt to produce, in such cases, spasm of the heart, cramp as it is so often called, which is so instantaneously fatal that the patient sinks, but is not drowned—he is dead before he sinks.

The drugs which will be found most useful vary with each case; pepsine in doses of ten or fifteen grains with each meal seems to do good always, but beyond that we must be guided by subsidiary symptoms. If there be much catarrhal irritation of the stomach, nitric or nitro-hydrochloric acid and calumba or quassia are often useful, soda, potass, or lime in the form of lime-water, often gives great relief, but never produces such a permanent effect as the acids; occasionally the alkaline treatment may be combined with the acid one with advantage, the acids being given before food, the alkalies from half an hour to an hour subsequent to a meal, and with these we may combine the use of podophylline in quarter-grain doses, with a third of a grain of ipecacuan and a quarter of a grain of belladonna, which in enfeebled patients unloads the liver, and relieves the right side of the heart without purging. If torpor of the liver be more marked, then small doses of blue pill and aloes just sufficient gently to move the bowels are most useful. If torpor of the colon be the chief apparent ailment, the long-continued use of Barbadoes aloes in small doses, with sulphate of iron, hyoscyamus, and nux vomica, answers very well; and if much flatulence be present, we may substitute a couple of grains of the compound galbanum pill for the hyoscyamus with advantage; or if gout be the fundamental ailment, then small doses of the acetic extract of colchicum with Barbadoes aloes, both in such doses as shall insure no more than one stool, a little more bulky or looser than usual, and continued daily or every second day for some time, will be found most useful.

When a hæmatinic tonic is required, as will be the case in most instances, the citrate of quinine and iron will be found to be the mildest, and the one most useful in all cases, while Easton's syrup of the phosphates of strychnine, quinine, and iron, is the most powerful, and if continued in drachm doses twice a day for several months, will often effect a most wonderful improvement in the patient's health, and in the state of his heart; that it may do so we must be careful to have all catarrh of the stomach removed in the first place, and the liver also acting freely, otherwise this tonic will not only do no good, but occasionally seems to do harm.

Whenever, from the state of the patient and the defective secretion of urea, gout seems to be impending, the most important remedy will be found to be the free administration of colchicum along with alkalies.

Although for temporary purposes there is no sedative equal to the subcutaneous injection of Squire's solution of the bimeconate of morphia, yet for continuous use as a nervine sedative bromide of potassium far surpasses it, but it must be given in full doses, from half a drachm to a drachm three times a day, till its full sedative effect is secured.

You will see then, that for the relief of intermittent and irregular cardiac action, we must endeavour first to determine the lesion upon which it depends, cardiac or otherwise, and we must treat this with due regard to the organic debility to which that lesion owes in injurious efficiency, and we must meanwhile not forget, that between the cause and its effect, we have the nervous system as a connecting link, and that by modifying or interrupting this connection, which we often can do by the judicious use of sedatives and narcotics of various kinds, we may cause to cease, or at all events mitigate the results pending our attempts at cure.

Cardiac palpitation is only too frequently dependent upon similar causes as irregular action, and is to be treated accordingly, especially by such means as shall restore a normal tone to the heart and to the organism generally. Now and then, however, an apparently accidental though violent attack of palpitation seems dependent upon acidity of the stomach, and can often be at once relieved by an antacid draught of soda, potass, or ammonia; and indeed not only palpitation, but also some of the minor forms of irregular action are promptly relieved by a draught containing a drachm of aromatic spirits of ammonia, with or without an equal quantity of tincture of valerian, or failing that, by a tablespoonful of whisky or brandy, with a teaspoonful of carbonate of soda, in about a wineglassful of water, just enough not wholly to drown the miller, as we say in Scotland.

Epigastric pulsation depending on irritability of the abdominal aorta is a local neurosis not always apparently dependent on dyspepsia, nor to be relieved by tonics. I have, however, found it almost invariably to yield to full doses of the bromide of potassium in some bitter infusion such as calumba, gentian, or chiretta. The only exception to this that I remember seeing was that of a woman, a patient in Ward XIII., in whom this excessive abdominal pulsation was accompanied by a preternatural hardness of that part of the artery, probably due to atheromatous disease, and in her case large doses of the iodide of potassium gave great relief, though nothing had any permanently curative effect.

In connexion with the subject of increased cardiac action generally, I may mention, that while increased action is liable to follow any unusual exertion, such as climbing a stair or going up a hill, both in hearts valvularly diseased, and also in those which are simply weak, palpitation or irregular action occurring while the patient is at rest is by no means to be regarded as a certain symptom that a heart is only weak or gouty, because of course hearts valvularly diseased are always weak, and often gouty, and therefore liable to present the symptoms of both diseased and also of simply feeble hearts. There is, however, one peculiarity by which the valvularly diseased heart may be perfectly discriminated from a simply weak heart, and that is, that while palpitation or cardiac discomfort occurring as the result of exertion in a heart valvularly diseased can never be relieved by anything but rest, the same results following exertion in the feeble heart of a nervous or gouty individual are frequently calmed down by any emotional excitement, especially of a pleasurable kind, such as meeting a friend, or the sight of anything novel or attractive, or even, strange to say, by a more violent exertion. Thus a man with a heart merely valvularly diseased is not likely to have any discomfort unless he meet with a slight ascent in his walk, when he is at once brought up and must rest; but a man with a gouty or feeble heart, though he too may be "afraid of that which is high," and may also suffer during the ascent, yet has his palpitation at once relieved by any emotional excitement, and if he be seized with sudden palpitation while walking slowly on the level, he will often find it disappear at once if he takes a short race to the next lamp-post: the heart beats the faster for the exertion, but the palpitation is gone, affording an example of a very peculiar form of inhibition, which probably only those can truly appreciate who have experienced it.

Part Second.

REVIEWS.

The Diseases of the Heart and of the Aorta. By THOMAS HAYDEN, F.K.Q.C.P., etc. Dublin: 1875. Pp. 1232.

FOR the last ten years Dr Hayden has been favourably known to the profession by his many excellent contributions to our knowledge of the diseases of the heart and aorta, and the present voluminous work is a monument of his painstaking and laborious research into the literature of the subject, forming a complete repertorium of almost every known fact as to the anatomy, physiology, and diseases of the heart and aorta from the time of Vesalius quite down to the present day. In this respect it is perhaps the most important work ever published, and one which cannot but be extremely useful to the student interested in the subject of which it treats, as in its pages they will find a reference to the discovery of almost every known fact, and a record of every opinion on any subject connected with the heart, of every physician or of every physiologist from Senae and Haller down to Hayden and Pettigrew, modern German literature being, however, but sparingly alluded to.

However valuable for the advanced student of medicine, it is, however, hardly a work to put into the hands of a tyro, or even into those of a busy practitioner; it is far too wordy, dilute, and indefinite for either of these. They do not want to read five-and-twenty pages, and study sixteen complicated diagrams, to ascertain what is the probable sequence of events in a cardiac pulsation; they must be taught *simpliciter* what is the most advanced and probably the most correct view—that of to-day; and it is only after a man has thoroughly mastered this that he begins to take an interest in the views of his predecessors, and in the development of the subject. But if we begin him at Lancisi, long before he has got to Hope, his brain is whirling, and his retina blurred by black figures and white lines, which seem continually interchanging, and convey to his mind no definite impression. Throughout the whole work the same fault prevails; Hayden himself is almost lost amid a host of opinions of others, all stated with the same equable impartiality, whether they emanate from some world-renowned physician, or from some mute inglorious Iapis, not known beyond the corner of his own street; and yet Hayden has very decided opinions of his own—not always, we think, correct, but still of importance, as emanating from a man of so much experience. In some cases his opinions seem to undergo a change as the work progresses; thus, at p. 64, he tells us that digitalis kills with the heart in diastole—a statement which, if true, would indicate it to be a powerful cardiac sedative like aconite; but by the time he has got to p. 525, he has become thoroughly persuaded that it is essentially a cardiac tonic and stimulant, inadmissible in cases of hypertrophy, and chiefly useful in softening and dilatation. There are also some curious statements made in the course of the work, one of the most singular of these being at p. 213, that the "bruit de rappel," or reduplicated second, is not pathognomonic of mitral stenosis, *because* at p. 865 he has related a case of double aortic murmur, in which *the second sound* was reduplicated, the *second element* in the reduplication being a *bellows-murmur.* But, in the first place, we doubt very much the possibility of accurately determining reduplication where one element is a sound, and the other a murmur, prolonged through both systole and diastole, as it is perfectly impossible accurately to define the exact moment of change from the one to the other, and the whole question of reduplication depends upon accuracy in this matter; moreover, in the case referred to, the patient did not die, consequently it is not known whether his mitral orifice was stenosed or not. The whole of his reasoning as to the cause of a reduplicated second may be accepted as a sample of what occurs throughout the work, in those places where he himself is permitted to appear. He first of all states what his own opinion is, and follows this up with, "if these views or observations are well founded, and I believe they are, it follows then," etc. This method of reasoning leads him to regard dilatation of a ventricle as a cause of delay in its emptying, and therefore a cause of delay in the closure of the valve of the artery which springs from it, hence reduplication of the second sound. But careful observation ought to have taught him that a reduplicated second is certainly not always or even often present when we have only to do with a dilated ventricle; that it is most common, as he indeed acknowledges, in mitral stenosis, and chiefly at that early period of the disease when there is no dilatation of either ventricle, while later on, when the right ventricle becomes dilated, reduplication is not so common, because the pulmonary intravascular pressure, which previously was great enough to close the pulmonary valve before that of the aorta, is now lessened by regurgitation through the tricuspid. While a further proof of the truth of what we have just stated as to the cause of the reduplicated second, is to be found in the fact, that when we hear it, and it is not always present, the pulmonary accentuation, audible at other times, is either much diminished or entirely removed, because when the sound is reduplicated the excessive intrapulmonary blood-pressure closes the valve in the face of an advancing current of blood, and the segments therefore fall more softly, as we can readily understand. All this is matter of observation, which Dr Hayden can verify for himself, and it is quite inconsistent with his theory of the cause of a reduplicated second sound.

Moreover, there is no organ of the body whose physical condition can be more accurately determined by physical means than the heart; there is therefore no organ in regard to which dogmatism or mere expression of opinion is less excusable, because there is no phenomenon connected with the discharge of its function either in health or disease, the cause of which, if it be not already distinctly known, may not be clearly ascertained by careful physical investigation, coupled with clinical and pathological observation.

But, indeed, Dr Hayden's experience seems to vary very considerably from that of every one else; thus, contrary to his compatriot Adams, he denies that irregularity of the pulse is characteristic of mitral stenosis, except in cases about to die. We find it often enough here in cases which recover, but we treat our cases, no doubt, somewhat differently from what Hayden does. He also, while acknowledging that the presystolic murmur is pathognomonic of mitral stenosis, says that "it is never permanently absent in cases of this lesion," and "that it is very seldom temporarily absent in such cases," adding, that it is only absent when the heart grows feeble, and chiefly when the patient is dying. Such views are not, however, consonant with the experience obtained either in Scotland or England, all observers—excepting Hayden alone, we believe—agreeing in stating that it is the murmur subject to most variations, most frequently absent, and some add that, in many cases of mitral stenosis, it has never been known to have been present at all, especially in cases occurring early in life, and associated with hypoplasia of the aortic system. But, indeed, Dr Hayden holds many peculiar views, inconsistent not only with the observations of others, but also with what we know to be actual facts. Thus, he says he has never heard a diastolic murmur in the arteries in cases of aortic regurgitation (p. 237); yet this is often very well marked; and he has also been unable to verify Henderson's observation, that the pulse is delayed in such cases—a matter of daily observation, and which can be ascertained beyond the possibility of cavil by the use of electric methods of tracing the pulse from the heart to the periphery, as devised by M'Kendrick and Landois. Yet, although he could so easily have ascertained the correctness of Henderson's opinion, Hayden is contented simply to state in regard to it: "I am satisfied it is erroneous,"—without any reason given beyond his own *ipse dixit*. Throughout the whole work this dogmatism may be traced: wherever the views or opinions of others are criticised, it is always with a reference to what he himself has observed, or what he holds to be correct; while the signal failure of his powers of observation to detect matters of such everyday occurrence as an arterial diastolic murmur in cases of aortic incompetence, or the delay of the pulse, which happens in every case of such disease, as well as the looseness and inconsequence of his reasoning generally, lead us to put but little confidence in the value of his observations, or in the picture he exhibits of the present state of cardiac diagnosis. We may make a somewhat similar remark as to the treatment propounded, which, even when apparently most advanced, is by no means sound. Thus, the use of ergotine in the hæmoptysis of mitral stenosis, though a novel method of treatment, can hardly, we think, be reckoned an improved one; while to teach that mercury, pushed to salivation, is the proper treatment of pericarditis, is really an attempt to bring back again the days when pericarditis was made a serious and too often fatal disease, in which we trust Hayden will have few followers.

In short, while this work is erudite in the extreme, in the sense of containing everybody's opinion, we not only fail to perceive in it any signs of advance either in diagnosis or treatment, or even any proper indication as to how such an advance is to be attained; but we are unwillingly compelled to acknowledge, that many of the statements bearing upon diagnosis are untenable and inconsistent with fact, while the treatment recommended for acute affections is as mischievous as it was thirty years ago, and that for chronic cardiac disease quite as hazy and indefinite as it then was.

Journal of the Scottish Meteorological Society, July 1875. Edinburgh: W. Blackwood and Sons.

The number of this Journal published last July, contains a Report "On the Influence of Weather on the Mortality from Different Diseases at Different Ages," prepared for the Society by Mr Alexander Buchan, its able and indefatigable Secretary, and Dr Arthur Mitchell, the Chairman of its Medico-Climatological Committee, which cannot fail to possess a very great interest for our readers, and to which therefore we desire to direct their attention. In this paper we have first of all discussed the distribution of deaths from different diseases over the weeks of the year, dealing with each cause of death separately, and in the order in which these causes of death appear in the Registrar-General's Report, and exhibiting graphically by means of a curve the results arrived at in the case of each disease. These calculations extend to every cause of death appearing in the returns of the Registrar-General, and the series of curves amount to fifty-five, comprising all the most important causes of death, the exact figures from which the curves are constructed being given in a tabular form at the end of the paper.

Afterwards those curves which show similarity are brought together and discussed in groups, the character of the weather coinciding with the maxima and minima being also carefully described. Attention is also directed to those curves which are opposed, and to those which have a direct or inverse relation to some particular element of the weather, such as temperature or humidity. Following this there is an inquiry into the influence of the weather upon the mortality at different ages. This branch of the inquiry is novel and interesting, and brings out some very definite results. Next an endeavour is made to show the influence of sex upon the mortality curves at different seasons, but from the returns of the Registrar-General containing no reference to sex except in the deaths from all causes, this part of the inquiry is not so satisfactory as it might otherwise have been. Lastly, the paper is concluded by a series of tables from which the curves have been constructed. The inquiry has been restricted to the mortality of London, because it affords an enormous population, confined to an area so limited as to have practically the same climate at each season, and also because in connexion with it alone could full weekly reports be obtained, reaching over a period sufficiently extended to give satisfactory results. The period over which this inquiry is extended comprises thirty years, beginning with 1845, and ending with 1874.

In the course of this most elaborate inquiry, which must have cost the authors an enormous amount of labour, many curious and interesting facts are brought out. Thus, Thrush, Atrophy, and Debility, and Want of Breast-milk, all show the diarrhœa curve in a well-marked form, as if to indicate that it is disorder of the bowels which in these diseases leads to the fatal issue. Erysipelas and Metria, Diphtheria, and Scarlet Fever, exhibit a close alliance with each other in the similarity of their mortality curves; while, strange to say, Hooping-cough disassociates itself from such apparently allied affections as Bronchitis, Pneumonia, and Asthma, and is found ranking with diseases of the nervous system, just where so many physicians think it ought to be. We have barely indicated a few of the points of interest of which so many are to be found in this paper, which we cannot too strongly commend to the careful consideration of our readers.

In conclusion, we may add that this important and elaborate investigation, which is done for all time, and will never need to be repeated, has been done as a labour of love, and out of devotion to the welfare of their fellow-men by its authors, who are officials of the Scottish Meteorological Society,—a society, the members of which, for the last twelve years, have devoted much time to this subject, and have published in their Transactions many interesting and important papers upon the influence of the weather upon public health. Yet for years they have endeavoured in vain to obtain a small grant from Government to assist them in their inquiries,—a Government which gives £10,000 a year to the Meteorological Committee of the Royal Society of England, who, whatever they may do for science generally, have done nothing whatever for the advancement of public hygiene, and whose own material has had to be utilized by the officials of a society which is unable to obtain one halfpenny of that money which the public so willingly expend for the public good.

Atlas of Skin Diseases. By Tilbury Fox, M.D., F.R.C.P. Part I. London: J. and A. Churchill: 1875.

There is no doubt that for many years past a good, small, or moderate sized, and moderate priced, Atlas of Skin Diseases, has been a great desideratum in the profession. There could not be a better foundation for such an atlas than Willan and Bateman's "Delineations of Cutaneous Diseases," upon which the present issue is based, and the publishers could scarcely have secured the services of any one better fitted than Dr Tilbury Fox, for bringing the pathology and treatment quite up to the level of the present time. The present Fasciculus contains figures of Erythema and Urticaria, four plates, and seven figures, all well done, and quite characteristic. The whole work will be published in monthly parts, and will contain seventy-two plates, of which one half will be original, and Dr Tilbury Fox's great experience and ample field is a sufficient guarantee that the new plates will be at once useful and instructive to the practitioner. This work needs no recommendation; it supplies in an efficient and cheap form a want which has been long felt; and it will repay its cost tenfold to the busy practitioner, supplying him not only with the means of recognising and naming any troublesome skin disease which may come across him, but also with a concise history of its causes, pathology, and treatment.

The Anatomy of the Lymphatic System: The Lung. By E. Klein, M.D., Assistant-Professor at the Brown Institution. London: Smith, Elder, and Co.

In this, the second volume of the work, Professor Klein publishes the results of his continued observations on the minute anatomy of the lymphatic system. The part before us is devoted to the elucidation of the origin and distribution of the lymphatics of the lung, and is divided into two sections; in the first of which the normal anatomy of these vessels is described, and in the second, the changes which occur in them under pathological conditions, such as those of inflammation and tuberculosis. According to our author, the pleura pulmonalis is covered by an endothelial investment of cells, which differ in appearance according as they are examined in a collapsed or distended lung, being columnar or polyhedral in shape, and separated at their free extremities by deep gaps in the former, whereas in the latter condition, the cells present the appearance of flattened scales which are closely applied to one another. The same fact is observable in the lung of a new-born child, when compared with that of a fœtus whose lungs have not previously breathed. The matrix of the pulmonary pleura consists, like that of other serous membranes, of firm bundles of connective tissue, separated by inter-fascicular spaces, each of which, being occupied by a flattened connective-tissue corpuscle, closely corresponds to the lymph-canalicular system of other serous membranes described by Dr Klein in the first volume of this work. The pulmonary pleura is closely applied to the superficial alveoli of the lung, and in some animals, more especially in the guinea-pig, is provided with a distinct muscular coat. The muscular bundles are developed to the largest extent over those portions of the lung which participate most freely in the respiratory movements; and, inasmuch as the lymphatics are distributed in the meshes of the muscular coat, and are in free communication with the cavity of the pleura, the author believes that by means of their alternate contraction and elongation during expiration and inspiration, what may be termed a pumping action is exerted on the contents of the pleural cavity, the progress of which into the lymphatics of the lung is thereby facilitated. The lymphatics themselves form a network corresponding to the intervals between the superficial lobules of the lung, and terminate in the bronchial glands. They communicate freely with the lymphatics of the lung itself on the one side, and with the cavity of the pleura on the other, by means of stomata. With such an arrangement, it is evident that the respiratory movements of the lung must exercise a material influence on the process of absorption from the pleural cavity, the stomata being thrown open during inspiration, and closed in expiration, the contents of the lymphatics being at the same time forced onward towards the deeper aspect of the lung.

The lymphatics of the lung proper consist of a peri-bronchial and peri-vascular system, the former accompanying the bronchi, the latter the branches of the pulmonary bloodvessels. In the walls of lymphatics of the first description, small masses of adenoid tissue resembling lymphatic follicles are to be observed, which agree exactly with the *peri-lymphagial* follicles described by Klein in his anatomy of the serous membranes. These peribronchial lymphatics commence in intercommunicating spaces of the mucosa, which spaces are lined by connective-tissue corpuscles, which are directly continuous with the endothelium of the lymphatic vessels themselves. With regard to the relation of these lymphatics to the bronchial tubes, Klein describes a form of nucleated branched cell, which is distributed among the ordinary epithelial cells of the bronchial tubes. These are furnished with processes, one of which is directed toward the mucosa, whilst the other projects into the free surface of the air-tube. These, which are nothing else than connective-tissue corpuscles, are connected with others lining the inter-fascicular spaces of the mucosa, and give rise to an arrangement such as has been previously described by the author under the name of *pseudostomatous* tissue, and by means of which the free surface of the air-passage is brought into direct relation with the inter-fascicular lymph spaces. As regards the perivascular lymphatics, they for the most part accompany the branches of the pulmonary artery and pulmonary vein, and originate, especially in the neighbourhood of the arteries, in a series of lymphatic spaces, which again, through the medium of pseudo-stomatous tissue, communicate freely with the alveolar cavities.

The sixth and succeeding chapters are devoted to the description of the changes undergone by the lymphatic system under pathological conditions. Upon injection of septic pus into the pleural cavity, germination of the endothelium of the pulmonic pleura takes place in patches, the groups of germinating cells being arranged round a central point. This point may sometimes be distinguished as a distinct aperture, whilst at others, it is occupied by a plug of fibrin; an appearance which is only to be accounted for by regarding the central point as a stoma. The germination of the endothelium is more evident in chronic than in acute tuberculosis. Moreover, the intermuscular lymphatic spaces, as well as the sub-pleural lymphatic vessels, become gorged with lymphoid cells, seemingly resulting from germination of the endothelial cells of these vessels, but also in part traceable to the emigration of blood-corpuscles. Along with this, we have the development of nodular growths, which are the result of the process referred to, in the superficial parts of the lung, the lymph vessels becoming converted into cords of adenoid tissue, the reticulum of which is in direct continuity with the endothelium lining them. In fact, we have here what has been previously described by the author as an endolymphagial development. These growths are common in the superficial parts of the lungs of animals, in which tuberculosis has been artificially produced.

With regard to the changes in the lung itself under these conditions, we have to note the formation of nodules, which are divisible into three classes, in accordance with their form and mode of development. The formation of two of these is traceable to modifications of the peribronchial and perivascular lymphatic tissue; whilst the third form is due to changes which take place in the alveolar tissue of the lung. "The appearance of perivascular adenoid tissue forms the earliest symptom of the process of artificial tuberculosis," and gives rise to the formation of the second class of nodules, which is the result of the development either of a reticulum of fibres lying in the intervals between a collection of lymph-corpuscles lying *outside* a vessel (perilymphagial nodules), or of a reticulum of fibres penetrating a collection of corpuscles *within* a lymphatic (endolymphagial nodules). Germination of the endothelium of the blood-vessels is also to be observed, and stands "in an intimate relation to the development of the perivascular masses." Of these three forms of nodules, the third alone is subject to cheesy transformation.

The last chapter of the work is devoted to the investigation of the histology of acute miliary tubercle in the human subject, and contains much original matter regarding the origin and development of the so-called giant cells, and the relation of these to the surrounding tissues.

We regret that the space at our disposal is too limited to admit of more copious extracts, but enough, we trust, has been said to enable our readers to perceive that the work will well repay very careful perusal on the part of those who are in any way interested in the pathology of lung disease. The text is illustrated by six plates, which are models of histological drawing.

Contributions to Surgery and Therapeutics. By J. C. OGILVIE WILL, M.D., Assistant-Surgeon, Aberdeen Royal Infirmary.

THIS is a reprint of five papers on various subjects, from different medical periodicals; one of which appeared in this Journal for April last. All the papers will be read with interest, and not without much profit, by members of the medical profession. Not the least interesting and instructive is the last paper, "On Croton-Chloral Hydrate, its Mode of Administration; Therapeutic Effects; and Action."

Thomson's Conspectus, adapted to the British Pharmacopœia. Edited by EDMUND LLOYD BIRKETT, M.D. Cantab., etc. New Edition, with a Supplement containing Notices of the New Medicines contained in the Additions (May 1874) of the British Pharmacopœia. London: Longmans, Green, and Co.: 1874.

THIS might have been a useful pocket work on the medicines contained in the British Pharmacopœia, had Dr Birkett been careful to give the new chemical formulæ correctly, and the chemical equivalents. He is in many cases perfectly at sea regarding the relation of the new to the old chemical formulæ, and in most instances he gives the chemical equivalents for the old notation. We could point out innumerable errors in regard to the formulæ of the various chemical substances, but this would serve no good purpose. Dr Birkett frequently uses capitals in giving the *specific* names of plants when they should not be used, and not unfrequently omits to use them when they should be used. It is a pity that writers on materia medica, and especially those who, like Dr Birkett, write for "*Students and junior practitioners*," should introduce the *old* chemical notation, for no "student" is now taught it, and few "junior practitioners" understand it.

This *Conspectus*, however, is not without its merits. The descriptions, doses, and uses of the various medicines are generally correctly given. In the *Appendix* will be found an interesting chapter on "some useful remedies employed on the Continent and in America," and also a useful chapter on *Poisons*.

Zoology for Students: A Hand-Book. By C. CARTER BLAKE, D.Sc., etc. With a Preface, by RICHARD OWEN, C.B., F.R.S. London: Daldy, Isbister, and Co.: 1875.

DR BLAKE'S volume presents both excellences and defects; and we deem it the strict duty of the reviewer, not only to his readers, but also to the author, to point out the latter with a view to their emendation in future editions of the work. Thus, the proof-sheets have certainly not been corrected by the author with the care which a scientific work specially demands. Even the printer's "reader" might, we think, have been more careful; but on Dr Blake himself lay the chief onus of ensuring the correctness of the technicalities of his volume. Thus, in page 336, we find "*Comatula*," printed "*Cornatula*;" "*Diphyes*" appears in page 345 as "*Dephys*;" "*Calynimidæ*," "*Calliancridæ*," and "*Pleurobraclidæ*," in page 355, should be "*Calymmidæ*," "*Callianiridæ*," and "*Pleurobrachiadæ*" respectively; whilst "*Hyalonemadæ*" is misprinted "*Hyasonemidæ*" in page 358. We regret exceedingly the presence of these errors in the terminology of a student's hand-book; for, as every teacher knows, learners are but too apt of themselves to grow careless and inexact in their use of technical names; and it is the duty of every educational writer to carefully guard on his part against being the inadvertent means of fostering mistakes. Even the evident misprint (page 378), of "sponge-shells," for "sponge-cells," might gravely mislead a junior student's ideas. Then, again, Dr Blake, has omitted to give us an index—that most necessary appendix to a zoological hand-book—and he has dealt very unequally with the invertebrate groups of animals, as compared with the vertebrata. We cannot help thinking that Dr Blake's labours would have been much more serviceable to students, had he confined himself solely to the description of the vertebrate sub-kingdom, and given us a full *résumé* of the latter group, along with a good index. He would thus have avoided the serious omission of the details of structure, etc., of many important invertebrate forms—*e.g.*, the *Rotifera*, of which interesting and anomalous group the only notice Dr Blake has given is contained in a passing reference in page 381. The illustrations are not sufficiently numerous, and do not deal with structural peculiarities sufficiently for the needs and wants of the student; and the titles of the woodcuts are given in a non-scientific manner (pp. 150, 151, and 297), and are sometimes omitted altogether (pp. 221, 224). The figure of Hydra (page 367), we think, is placed upside down; although certainly a Hydra looks almost as well the one way as the other. But enough of this fault-finding, which is by no means a pleasant task, even when conducted in the kindest of manners. We are prepared to find Dr Blake a warm supporter of the veteran Owen's views and opinions, and in the classification of the various groups our author has followed in the footsteps of his master. We hardly know whether or not, in introducing Owen's cerebral classification of the Mammalia, Dr Blake has excluded from his manual all "problematical classifications," as he tells us in his preface. For, as every one knows, the battle of mammalian classifications has not been a short or light contest, and the student as such, in our opinion, would have profited more from a classification founded on more ascertainable and commonplace grounds—such as the structure of the placenta or generative organs. However feasible theoretically Owen's claims for his classification may be, as set forth in the introductory note to Dr Blake's volume, there can be no doubt that, to the mere zoological learner, it will seem of an abstruse character. Students require to be very well "up" in the anatomy and physiology of the brain, to thoroughly appreciate and to do justice to Owen's system. Dr Blake's descriptions of the vertebrate orders are for the most part clear, if they are somewhat too technical for junior students; and in many places we can detect the special teachings of Owen as set forth in his splendid treatise on Vertebrate Anatomy and Physiology. The extinct reptilian groups are fully described, and present perhaps the most lucid part of Dr Blake's book. To the advanced student with a solid foundation of general morphology and physiology, we can recommend this manual for the study of the vertebrata. The printing is clear, and the publishers are to be commended for the care with which their portion of the work has been executed.

The Student's Guide to Human Osteology. By W. W. WAGSTAFFE, B.A., F.R.C.S., Assistant Surgeon to, and Lecturer on Anatomy at, St Thomas's Hospital. London: J. and A. Churchill, New Burlington Street: 1875.

IN Wagstaffe's Student's Guide to Human Osteology, we have an addition to works upon Osteology, which will be much appreciated by students reading for examination. It is convenient in size, and the matter is brief and most methodically arranged under definite heads, so that reference can instantly be made to any point connected with a bone; and at the end of each description a summary of points of importance is appended. The articulations and development are always described, and different kinds of type have been freely used to mark parts which have received special names, or to facilitate reference. The descriptions consist of little more than classified catalogues of points connected with each bone, written without the smallest guidance from morphology, and are as dry and uninteresting as they well could be. A novel and excellent feature in the book is a tolerably full account of the arrangement of the rods and plates of the cancellous tissue in relation to resistance to force, illustrated by many praiseworthy woodcuts representing the internal structure of bones. The difficult subject of the mechanism of the various bones is treated at considerable length; indeed, this and the internal structure may be said to be the prominent features of the book, and sometimes cover more pages than the ordinary description. There is a chapter on the skulls of different races of mankind, and the book concludes with a useful account of the various bony landmarks perceptible in the living body. To show that the illustrations are ample we need only say that there are twenty-three double-page lithographic plates, and sixty-six woodcuts, on the former of which the origin and insertion of muscles are shown by entire and broken red lines. A few of the figures in the plates give little help to the text, but in general they are well executed. On the whole, we must confess to a feeling of disappointment in regard to this book; for though well adapted to refresh the memory of the senior student undergoing the process of "cramming," we should regret to see it in the hands of any one to whom a scientific knowledge of osteology was important.

Remarks on the Origin, Varieties, and Terminations of Idiocy. By GEORGE WALLINGTON GRABHAM, M.D. Lond. Earlswood, Red Hill, Surrey: 1875.

The Province of Psychology. By Mr Serjeant Cox. London: 1875.

Modern Spiritualism. By W. J. MARSHALL, M.D., L.R.C.S. Ed. Greenock.

The Skull and Brain, their Indications of Character and Anatomical Relations. By NICHOLAS MORGAN, Author of "Phrenology, and How to use it in Analyzing Character," etc. London.

DR GRABHAM tells us that his information has been somewhat hurriedly got together in the intervals of routine work.

The paper consists of a series of remarks which have occurred to him during his superintendence of Earlswood. This Asylum is full of interesting cases, and from the great number of inmates, the number of deaths is so considerable that it is a pity we have not more frequent contributions to the pathology of idiocy. Nevertheless, what we have got is both interesting and valuable. We think this can be best shown by a few extracts. Dr Grabham remarks the well-known fact, that male idiots are commoner than female, and is disposed to attribute this to their greater liability to injuries at birth. The following considerations, however, do not tend to bear out this view:—

"Dr J. Langdon Down, in a paper on 'Some of the Causes of Idiocy and Imbecility,' published in the *British Medical Journal* of 11th October 1873, gives the 'ratio of sex among idiot primiparæ as 3 males to 1 female.' Now, assuming from the relative proportion of the sexes in the Earlswood Asylum, that male idiots are twice as numerous as female, we should expect to find the first-born in the same ratio, if there were no additional risk to the male child at birth; but taking the percentage of first-born of the idiots of each sex, what do we actually find to be the case? The records of 1100 cases show that 23 per cent. of the males were first-born children, while the females amount to nearly 25 per cent. This is not by any means what we should have anticipated, and rather indicates that greater importance must be attached to maternal accidents and impressions during pregnancy, as causes of idiocy in the first-born. Again, if it be true, as has been stated, that the average number of children in an English family is less than four, the proportion of first-born must be as great with sane chilrden as with idiots. It is probable, however, that the average family in which an idiot child occurs is more numerous than this."

In reference to the statement in the two last sentences, Dr Grabham should have taken the trouble to ascertain the average number of children to a family in England, as well as the fecundity of those families in which idiot children are born. Doubtful statistics make doubtful conclusions. The following passage describes some peculiarities about idiots:—

"A spongy and swollen state of the gums is frequently seen, also irregular decaying teeth and offensive breath. The secretions of the skin have likewise a peculiar, unpleasant odour. Vegetable and animal parasites find an appropriate nidus for their growth and multiplication. Undue brittleness of the bones is not uncommon. I have seen the femur and the humerus broken during the struggles caused by an epileptic fit; and I have found the long bones generally reduced to a mere shell after death. In many patients, general sensation is very low in degree, the extraction of a tooth causing little or no pain. I have, more than once or twice, seen a comparatively intelligent boy sit quietly in a chair while his toe-nail was removed; requiring no one to hold him, and uttering no exclamation; but looking on as if interested, and stating that the operation did not hurt him. A child, who had severely burned his hand by holding it in a gas flame, took the first opportunity after recovery to endeavour to renew an experience which to him did not appear painful. Verily, the burnt child does not always dread the fire."

We doubt if the brittleness of the bones is at all a common characteristic of idiocy. There are a good many idiot savans at Earlswood in whom one or more faculties are amazingly developed. "One has a marvellous power of acquiring languages and musical knowledge; another, great mechanical skill and original constructive ability; a third, though very childish, is no mean mental arithmetician; a fourth remembers all he reads; a fifth delights in dates; a sixth can tell the time when awakened from sleep while a seventh is an excellent copyist of engravings."

The pamphlet is illustrated by seventeen very good heliotyped photographs, and has been reprinted from the *British Medical Journal* by the inmates of the Asylum.

Mr Serjeant Cox shows great power of argument in his address on the Province of Psychology, as may be seen from the following passage :—

"How, therefore, do the physicists construct their sciences of Electricity and Magnetism? Not by seeing or feeling the imperceptible fluids, if such they be, but by noting the effects they produce upon the molecular structure the human senses are enabled to perceive, and thence deducing conclusions, more or less probable and more or less conjectural, as to the nature of the forces, of whose existence they do not entertain the slightest doubt, although they are not actually known and probably are unknowable. By precisely the same processes as the Materialists employ for ascertaining the existence, the powers, and the properties of those unknown and unknowable things, Magnetism and Electricity, do the Psychologists propose to ascertain if there be in the human organism, or associated with it, or in any way controlling it, something as imperceptible to the senses, and consequently as unknown and unknowable, as are Magnetism and Electricity—some entity—be it a force or a thing—such as that we call *Life*, which is in organic structure only—or that which is called *Mind*, and which is found only in animal structure; and if there be not also something, other than *Life* and *Mind*, that gives to Mind its consciousness of individual being—something that constitutes the Man and yet is distinct from the body of the man, which we call Spirit or Soul."

We remember Dr Alison used to say: We know as much of vital forces *as of any other.*

Dr W. J. Marshall, of Greenock, in a pamphlet of 52 pages, exposes the pretensions of the spiritualists. The work seems to be published for the public, though medical men will perhaps read it with most interest. We cannot say we agree with everything in Dr Marshall's work, but we have read it both with profit and interest. We can only spare room for a single paragraph.

"In conclusion, I would beg my hearers to remember that the phenomena of modern spiritualism only occur in connection with a morbid state of the nervous system in one or more of the persons engaged in a spirit seance. I would warn them that this is a state in which the reason and will are, as it were, asleep, and the automatic action of our frame has full play—that it is a state which is allied to trance, hysteria, dreaming, somnambulism, and insanity—that it is a state which is dangerous, as it is opposed to sound mental and bodily health, and one which ought, on all these accounts, to be sedulously shunned. It is a well-known fact, that many votaries of mesmerism found their way into lunatic asylums, and I have no doubt many spiritualists will follow their example—indeed, one spiritualist naively remarked, that the insane were good mediums."

We consider Mr Morgan's book, on the Skull and Brain, a mere instance of perverted ingenuity. It is likely that there are still people who may require to have it explained that phrenology is a refuted delusion, but we may imagine very few of our readers are in this condition. Those who are, may be left to Mr Lewes and other makers of books on what is called popular science. The time has gone by since the Combes defended Phrenology, and Sir William Hamilton and Lord Jeffrey attacked it. It will die out without any further discussion, having been universally abandoned by physiologists.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LV.—MEETING II.

Wednesday, 1st December.—Dr GRAINGER STEWART *in the Chair.*

I. *Mr Joseph Bell* read a paper ON OPERATIONS FOR RELIEF OF EXTENSIVE DESTRUCTION OF THE VESICO-VAGINAL WALL, which will appear in a future number of the Journal.

Dr Grainger Stewart felt sure that all the members must have listened to Mr Bell's paper with great interest. The relief afforded to the unfortunate sufferers must have been invaluable to them. The plan by which Mr Bell had obtained success in such difficult cases was one that recommended itself to him; and he would be glad to hear the opinions of the surgeons and obstetricians present.

Professor Simpson was sure that the members felt much indebted to Mr Bell for his very interesting communication; for although cases of vesico-vaginal fistula were much rarer now than formerly, they yet, from time to time, were brought to the Infirmary. Most of them were easy; and if, as sometimes happened, the first operation failed, complete closure was, as a rule, effected in the third at the utmost. He had now operated in many cases—how many he could not at present exactly say. He had, however, never met with cases so difficult as some of those narrated by Mr Bell. In one case in his late uncle's practice, the vesico-vaginal wall had been so much destroyed, that there was no prospect of any benefit being afforded, unless by complete closure of the vagina. The patient was a girl of sixteen, in whom the vesico-vaginal septum had sloughed, so that no ordinary operation would suffice. The genital fissure was then closed by the labia being made raw and brought together with iron wire. She did not afterwards wear a catheter, but a boat-shaped shield instead, which kept her in comparative comfort. The only case where he had himself been obliged to close the vagina was in a married woman on whom he operated last summer, where the uterus was found to be tilted by the pressure of cicatrices into the cavity of the bladder. The patient had been in the wards of the Infirmary two or three years previously. The case was an extremely puzzling one at that time; but the difficulties were cleared up by her suffering from an abortion at the third month. She was accordingly then sent home with orders to come back when the parts became more healthy. It was only, however, last summer that she did return. It was then found that the sloughing had so taken place in the vault of the vagina, that the cicatrix had pushed the uterus into the bladder. The fistulous opening admitted the tip of the finger. From the extensive sloughing, there was no hope of so closing the fistula as to bring back the os uteri into the vagina; and it was therefore explained that any operation would necessarily put an end to conception. The operation consisted in bringing the anterior and posterior walls of the vagina together. One difficulty was that the speculum could not be used in the introduction of the stitches; and there was great risk of the peritoneum in Douglas's space being wounded. Whether it was or not he could not say; but at any rate the aperture closed perfectly. The patient remained under observation for two menstrual periods. In the first there was pain; but the result was a perfect cure, the menstrual fluid passing by the urethra. The vagina admitted the finger as far as to the second joint, but it ended in a cul de sac, through which there was no communication with the os uteri. In another patient from the Highlands, the fistula was not large, but the operation for her relief was rendered very difficult from the circumstance that the minute orifice was seated at the bottom of a deep groove. There had been exfoliation from the surface, and shortening and narrowing, so that the finger filled the vaginal orifice. There was, therefore, considerable difficulty in exposing and closing the orifice, so that a second operation was necessary. She was now one of their Infirmary nurses. Such operations required patience, but gave great satisfaction, as Mr Bell had so often experienced. He was glad to hear such cases, and the success which had attended them would be an encouragement to other operators to close up the vaginal orifice in those cases of extensive genito-urinary fistulæ, where no other method of affording relief could be adopted.

Dr John Duncan thought that there was one point in Mr Bell's paper which deserved more than a passing notice. He himself had no experience in vesico-vaginal fistula, as he had only operated once; and he had never seen cases so extensive as those given. The method used by Mr Bell, viz., that of not removing any tissue, was one, he thought, deserving greater extension. He believed it might be employed in many cases of vesico-vaginal fistula as well as in other surgical proceedings. Five or six years ago, in a case of preternatural anus, where the gut in a hernia had become gangrenous before operation, he found that, after destroying the septum, a plastic operation was necessary. It occurred to him that a better method than the usual unsuccessful way would be one on Mr Bell's principle. He therefore, instead of exposing a raw surface to the irritation of the fæces, determined to interpose mucous membrane. He accordingly split up the bowel all round, stripped off the mucous membrane, and brought it together by means of catgut; wire being used for external parts. The result was, that it all healed except a pin-point opening, which resisted the red-hot wire, as well as a plastic operation, but finally yielded to a second cauterization. He therefore thought that the principle was a good one, deserving of extension.

Mr Bell had little to add to what had been said. He had listened with extreme interest to Professor Simpson's cases. An interesting point in one of them was that retroversion of the uterus had begun prior to operation. He had hoped for this in one of his own cases. He was glad that Professor Simpson had experienced the same difficulty with the speculum as himself. In his case, he thought it had been due to his own awkwardness. As to the catheter, he thought it was important. He made it as long and curved as possible, and therefore made the urethral opening as near the mons veneris as he could. The catheter must balance itself. The case of fistula at the bottom of a groove, and in a small vagina, must have been of extraordinary difficulty. In one case of his own, by no means so bad, the operation occupied two hours. In regard to Dr Duncan's remarks on the principle on which the operation was based, he thought he must have got it from Dr Duncan's paper on the case of artificial anus. He had applied it in cases of ordinary vesico-vaginal, of harelip, and of cleft palate. He noticed that Mr Maunder had lately published, as a novelty, a case in which he had adopted the splitting plan for reuniting a ruptured perineum. He had operated by the same method at least ten years ago, and several times since, in ruptured perineum. It certainly appeared quite new, but, like many novelties, it was really an old idea.

II. *Dr Cuthbert* then read A CASE OF TORPOR OF THE LIVER AND BOWELS FROM BIRTH, AND CONSEQUENT PERSISTENT CONSTIPATION, IN A BOY SIX YEARS OLD; RECOVERY; which will appear in a future number of the Journal.

The President remarked that a case so peculiar was well worthy of the Society's attention. All must have remarked the accuracy and clearness of the description given; and he would be glad to hear any further observations on the subject.

The Society then proceeded to private business and election of office-bearers.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXV.—MEETING I.

Wednesday, 17th Nov. 1875.—Dr MATTHEWS DUNCAN, *President, in the Chair.*

Dr Hermann J. Groesbeck, and Dr Porteous, Pathhead, Fife, were elected Ordinary Fellows of the Society.

I. *Dr James Young* mentioned a case in which the PLACENTA was diseased in its whole extent, having undergone fatty degeneration; the child, however, had lived till within three or four days of its birth; the liquor amnii was natural.

II. A paper on SARCOMA UTERI, by *Professor Simpson,* was read, which appears in this Number of the Journal, at p. 577.

Dr Keiller thought the paper one of value and novelty. He could, on looking back, remember a number of such cases, one of a somewhat suspicious character he would refer to. He had reported it to the Society as a fibroid growing from an inverted fundus uteri, and had removed it as such; the woman recovered, and still survives with the uterus inverted. In this case he pulled the tumour down to the vaginal orifice, and saw the inverted fundus with the tumour attached to it. After removal of the tumour by the écraseur all efforts to re-invert the uterus failed, though great force was employed. The patient for a long time subsequently had severe periodic menorrhagia, but little or no pain or tenderness. Some other cases he regretted having interfered with, and now thought he would have done better to leave them alone. He had often injected the perchloride of iron in such cases, and had found much benefit from it; the longer he lived the more he felt disposed to use palliative rather than radical treatment in cases of a malignant nature, which were often aggravated by interference, and seldom or never cured by operative treatment; although it must be admitted that the knife and other so-called radical measures are occasionally called for in order to diminish hæmorrhage, or to relieve other urgent symptoms.

Dr Gordon was struck, in Mrs B.'s case, with the very slight tendency to bleeding in spite of the great size of the surfaces which were cut through. He was quite satisfied that her life was much prolonged by the repeated operations. He considered that they added several years to her life.

Dr Duncan had found it very hard to satisfy himself about the classification of the various malignant diseases of the uterus; he called them all malignant disease. No doubt, Virchow's definition and description of sarcoma were of great importance, but we must remember that they were denied by another school of pathologists. Clinically he found the greatest differences in their histories and symptoms; and that not only in tumours growing from the uterine wall, but in those affecting the os uteri also—some were distinctly cauliflower, others were spindle-celled sarcoma; between them there was an infinite variety, so that he could come to no conclusion as to classification. Dr Simpson's cases were all sarcomata according to some definitions, but they differed materially in their history, course, and symptoms. The Society was much indebted to Professor Simpson for his valuable paper.

Professor Simpson, in reply, observed that in the cases he had related the disease certainly varied in character; in Case I., there was a distinct descent from fibroid to sarcoma, while Case IV. may have been one passing from sarcoma to carcinoma. At the same time, two of the cases were typical illustrations of a definite form of tumour. In attempting to classify the mixed forms of growth, not only the anatomical structure, but also the clinical history must be taken into consideration. In Dr Keiller's case the tumour removed was not a sarcoma, but a fibroid tumour, and was, in fact, an illustration of the rare complication which he thought he had referred to in his communication, of which there are several instances on record, where a uterus containing a fibroid polypus had became inverted. Its complete removal without recurrence certainly was in favour of the view that it was a fibroid. The patient had lived a long time since the operation, whereas one of the great features of sarcoma was its tendency to recur.

SESSION XXXV.—MEETING II.

Wednesday, 24*th Nov.* 1875.—Dr PEEL RITCHIE, *Vice-President, in the Chair.*

I. *Dr Underhill* showed a UTERUS, deeply embedded in the cervical wall of which was a cyst about the size of a large filbert. When cut into, it was found to contain a clear transparent yellowish glutinous fluid.

II. *Mr Jamieson* exhibited a PLACENTA, in which the vessels forming the umbilical cord divided before they reached the edge of the placental structures, the so-called insertio velamentosa.

III. *Dr Peel Ritchie* showed a POLYPUS nearly as large as a walnut, which came from the rectum of a child. He remarked that, though hæmorrhage from the bowels was stated by authorities on children's diseases to be frequently caused from the bowels by polypus, such was not his experience at the Children's Hospital here.

IV. *Dr Rattray* showed an UMBILICAL CORD, 42 inches in length, which had not been wrapped round any part of the child, either neck, trunk, or extremities.

V. *Dr Bruce* showed an INSTRUMENT which he had devised for the performance of vaccination, and which consisted of a pencil-case handle with a needle at the one end to be used as a scratcher, and a flat blade for applying lymph at the other, both sliding into the case when not required. The advantages of this instrument were, its extreme neatness and handiness, and the almost invariable success which attends its use, as out of 340 vaccinations failure was only once met with. Besides, the needle could easily be kept clean, or replaced by another if required.

VI. *Dr James Young* then read a paper on LACERATION OF THE PERINEUM, which appears at page 607 of the Journal.

Dr Carmichael agreed with Dr Young in his treatment. In his earlier days of practice, he had not been prepared to stitch the perineum at once in cases of severe rupture; but now he always took the necessary materials with him. He had found that patients of considerable age had a great tendency to suffer laceration. He found the use of hot sponges to the tensely stretched perineum of great service in assisting expansion.

Dr Peel Ritchie had also found great benefit from the application of the hot sponge to the perineum, and had never found any bad consequence from it. His own experience of serious rupture was not very great. He was glad to hear Dr Young advocating the early stitching up of such cases. Dr More Madden, in a paper read before the Society some years ago, had advised a more elaborate method of applying the sutures, but he thought the simple plan was the better.

Dr Macdonald agreed with the preceding speakers as regards the value of early application of sutures. But he preferred wire to catgut sutures, and found the wounds healed readily with them. In his experience, and he happened, curiously, to have been called upon to operate for this accident three times within the last three weeks, if the perineum were soon sewn up and the patient kept quiet, there was little risk of fever, and the cases do surprisingly well. In a case which he had seen some time ago, where there was a contracted pelvis, the head and hand presenting, he had put on the forceps without any one to help him, and a sudden pain drove the hand and head together out of the vulva, tearing the perineum severely, the rupture extending an inch at least up the rectum; it was sewed up twelve hours afterwards, and healed well. He gave no opium, as the bowels are generally confined during the first week without it. Nor did he think there was any need of using the catheter. Many women go through life with hardly any perineal support in consequence of a little lack of courage on the part of their medical attendants. He considered that the difficulties in the way of successful operation in bad perineal tears were greatly exaggerated in the minds of the profession; and much comfort would result to patients if every bad rupture were sewed up, either at the time of its occurrence, or within the next twelve hours or so, according to convenience.

Dr Pridie mentioned a case in which laceration had taken place during the expulsion of the shoulders, the head having come through without doing any injury.

Dr Ballantyne supported Dr Young's views as to the advisability of early dressing. In a recent case of cross birth, where there had been a tear, he had sewn it up at once, and the patient had had a child since without a repetition of the laceration; he had often found a small tear caused by the head made worse during the passage of the shoulders.

Dr Bruce thought prevention was better than cure. In employing the forceps, it was of great convenience to direct the head well forward when it stretches the perineum, and to remove the instruments before the termination of the labour. In the majority of cases of laceration, he thought nothing need be done beyond cleanliness and attention to position; if sutures were necessary, probably they were better applied at once.

Dr Burn agreed with Dr Bruce on removing the forceps, when it is only the perineum which prevents the termination of the labour. He had practised midwifery since 1824, and had only once had a bad tear into the rectum; it was sewn up and did well; the tear was repeated in the patient's next labour, and she is going about and has perfect command of the bowel. The rapid forcing of the head on a rigid perineum was in his experience a frequent cause of rupture.

Dr Rattray had adopted the plan of removing the forceps, when the head was on the perineum.

Dr Menzies thought that there was great risk of rupture in first cases, where turning is necessary, and the child has to be extracted rapidly to save its life.

Dr Milne had seen but little of torn perineum; he had used forceps very often, but thought he had had more tears in cases where no instruments had been used. He found most of the cases heal well if left alone, and had only once had to sew; in that case the tear was through the sphincter.

Dr Simpson remarked, that no one had yet referred to diseased conditions of the perineum as a cause of rupture; such, for instance, as the soft friable state of the tissues, sometimes produced by syphilis. A case of this kind had happened in the Maternity a few years ago. Distortion of the pelvis was another common cause, of which the two worst varieties were the flat sacrum and narrowing of the arch of the pubes; the former preventing the proper sweep of the occiput, the latter forcing it far down towards the anus. As to rupture in forceps cases, he was not sure that removing them before the head was quite extracted was of any service in preventing injury. In a recent case, he had removed one blade, but could not get the other away, as it was caught over one of the ears; in this case there was a slight laceration. The worst case he had seen in his own practice was in a primipara, the head being in the occipito-posterior position; the tear passed entirely through the sphincter; this was not sewn up till the next day, as he had not the opportunity of stitching it immediately; but it should always be done at once, within thirty-six hours after the accident at latest, when the tear passed through the sphincter. All other cases he left alone; if not done at once, the operation for repair of the perineum should be delayed until lactation is accomplished, as the surfaces sometimes will not unite earlier.

Dr Young replied.

Part Fourth.

PERISCOPE.

J. ORTH ON TUBERCULOSIS. *Berlin. Klin. Wochenschr.*, No. 12.—Supported by post-mortem observations and experimental investigations, Orth maintains the truthfulness of the theory of the inoculability of tubercle. He gives a short resumé of seven cases, in each of which there were cheesy tubercular central points—in the pulmonary parenchyma, in the neighbourhood of a bronchus, in the bronchial glands, in bone, or the intestine. In the immediate vicinity of this central mass there was further tubercular deposit, thickest towards the centre, and more sparing towards the periphery. The relation of the peripheral tubercles to the central focus was shown by their arrangement, their being grouped round the centre, their character as recent products, and the transitions from the small gray transparent nodes of the periphery to the older yellow nodes of the centre. Specially instructive is a case of pulmonary phthisis, in which there were numerous isolated tubercular foci, amongst them, one undergoing calcification, and tubercular ulcers in the intestine, and around each centre abundant formation of tubercle had taken place. One of the intestinal ulcers was in the rectum, at the part corresponding to the recto-vesical fossa, and had extended close to the peritoneal coat. There had been produced in consequence false membranes—of some standing at the time the patient died—between the intestine and the bladder, and there were fresh tubercles in the false membranes.

Friedlander's view, that in such cases the tubercles in false membranes are deposited simultaneously with those which produced the ulcer in the intestine, is contradicted by the case cited above, as it is evident that the false membranes were due to the ulceration of the intestine produced by the tubercle primarily deposited there. Orth believes that general infection of the system from one or several such centres, especially those which are breaking down, takes place through the bloodvessels. The localized extension of the tubercle, he believes to be produced not only by the mechanism of the bloodvessels, but by the lymph and chyle channels, and by simple contact with a tubercular surface.

In the experimental part of the investigation, Friedlander's scepticism regarding the identity of inoculated tubercle with ordinary tubercle, is opposed. In conclusion, a case is adduced in which cheesy tubercle was introduced into the peritoneal cavity of a cat. First, where the omentum had adhered to the wound, and then on other parts of the parietal and abdominal walls, discrete tubercles had formed, which, when compared with tubercle from a phthisical subject, examined fresh, showed a perfect similarity.—*From the Centralblatt*, No. 52, 1875.

A. HILLER. *An Experimental Investigation regarding the Organized Nature of Contagia and Putrefaction* (*Langenbeck's Archiv*, xviii.).—Starting from the view that the parasitic influence of micrococci organisms in septic infection will only be decided should it be possible to introduce these organisms into the animal body free from any chemical admixtures, Hiller isolated as perfectly as possible, by methods which he has previously described, bacteria of all kinds, sometimes cultivated by himself, sometimes from blood-serum, flesh infusions, urine, and other putrefying substances, and kept in distilled water. He found such bacteria could be cultivated when introduced into an appropriately prepared medium.

From 0·5–1·0 cub. centim. of the water, containing the different isolated bacteria, was injected daily for nine consecutive days into the subcutaneous tissue, or muscles, or into the veins of rabbits and dogs. In more than eighty such experiments, he never saw inflammation, suppuration and fever, or any prejudicial effect follow as a consequence, although there might be some passing œdema. Suppurating wounds sprinkled with these isolated bacteria, were not noticably affected for the worse, and subcutaneous wounds did not suppurate, even when thousands of living bacteria were introduced into them. Notably, there never occurred after the most varied forms of application of these organisms to dogs and rabbits, any of those constitutional conditions which are known to complicate wounds. An inoculation which Hiller made on himself by subcutaneous injection into the forearm, with the exception of some œdema, was followed by little reaction. Accordingly, Hiller denies that there is any independent mechanical parasitic action, or that bacteria can thrive in living and healthy tissue, or blood. He considers that he has refuted the objection, that in his experiments he had always injected "innocuous" and never purely "pathogenic" bacteria.

Thus Hiller, after an examination of all the results hitherto obtained, will not allow that bacteria have any other rôle in septicemia than that of being in virtue of their smallness, ubiquity, and special relation to septic action, a frequent, nay constant accompaniment of constitutional wound-complications, as has been shown by anatomical investigation; that they further act as carriers of the septic poison, inasmuch as they imbibe the poisonous substances of putrid matter, and can thus produce inflammation, abscess, and gangrene; finally, that they "perhaps" produce or reproduce the septic poison, the possibility of their doing so, not being excluded by the isolation experiments.

In his experiments regarding putrefaction, Hiller found that a number of eggs, into some of which he injected, with proper precautions, fluid with bacteria isolated from putrefying substances, and into others, fluid with bacteria cultivated for the purpose—fifteen drops being injected — remained fresh after 10–14 days, but putrefied rapidly when air was admitted. He concludes, with Billroth, that bacteria are in themselves incapable of decomposing albumen by assimilation. He believes that in atmospheric dust, besides bacteria and other living germs, there must be other dead, corporeal, and organic matter, which constitutes the ferment of putrefaction, which might be either a ferment formed from cells or protein substances in the act of decomposing, as Liebig imagined. He proves this by the following experiment:—He poured boiling distilled water on dust collected in his room, and allowed hen's eggs to soak in it, and added it to a solution fit for the development of bacteria, but which contained none. Of the four soaked eggs, only two putrefied, one quickly, and one slowly; whilst bacteria isolated from the abundant development which took place in the solution, were injected into eggs without producing putrefaction. From the fact that the dust-infusion itself after several days showed signs of putrefaction, with development of bacteria, vibrios, and afterwards infusoria, Hiller concludes that atmospheric dust must, in fact, contain putrefiable substances.—*From the Centralblatt*, No. 49, 1875.

THE *Journal of the Scottish Meteorological Society* contains an elaborate paper on the Influence of Weather on Mortality from different diseases and at different ages, by Mr Buchan and Dr Arthur Mitchell. The field of observation and study is London, where a very large population occupies an area so small that the climate may be held to be the same for all. A great number of facts are brought out, which it would be very difficult to generalize further than has been already done by the authors. The highest death-rate is in January and December, the lowest in June. Smallpox causes most mortality in May; measles in December and January; scarlatina in October; whooping-cough in April; typhus fever in January, February, and April; and typhoid in November and December. Diseases of the bowels and cholera are commonest in the hotter months; diseases of the air-passages and of the kidneys in the colder ones. Asiatic cholera seems to be subjected to climatic variations as it is in India. In the words of the paper:—

"15. *Cholera.*—This curve is represented on Fig. 15 by the regularly dotted line, and shows certain irregularities of form, due to the outbreaks of Asiatic cholera in 1848–49, 1853–54, and 1856, thirty years being too short a period to furnish data for a smooth curve for a disease which is liable to be so greatly increased by epidemics recurring at distant intervals. The curve begins to rise in the beginning of June, rises rapidly in July, maintains a high but comparatively steady position in August, and again in the first two weeks of September, shoots up to an average 548 per cent. above its weekly average on a mean of the year. After this time it falls at a greatly more rapid rate than it rose. Its absolute minimum occurs during April and May."

Convulsions and nervous diseases generally have their highest mortality in March and April. Mortality, however, is influenced by many other things besides the weather, and perhaps the inhabitants of a large city are more independent of climatical conditions, and less exposed to the out-door influences, than the inhabitants of the rural districts. On the other hand, we should judge them much exposed to the mortality derived from defective diet and unhealthy occupation. The authors point out the enormous death-rate amongst infants from diarrhœa during the hot season of the year, and think it may be due in part to the scarcity of milk. It may be fairly held that there is no such thing as milk in London. We do not know what the white fluid sold under its name at high prices really is, but we are sure it is not milk. We think that the authors might have noticed that the increased number of deaths from purpura and scurvy from March to July, is probably owing to the want of potatoes and fruit during these months.

The authors remark that the general resemblance of the curve for pericarditis and for rheumatism deserves consideration. Surely this is not difficult to explain. Rheumatism does not appear of itself as a frequent cause of mortality, but it often brings on death by inducing heart disease. Altogether, this paper is well worthy of study. It is evidently compiled with an enormous amount of labour, and a due admixture of thought. It would be a great service to science if the authors, having gone on so far, were to make an investigation on the mortality of London from all causes whatever; and in doing this, we do not think the work would be so greatly increased as it might appear at first sight.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. HANDSEL GRIFFITHS, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal*.]

ACIDUM ARSENIOSUM IN PALUDAL FEVERS.—Sistach corroborates the opinion expressed by Fremy, Fuster, and Girbal, that the febrifuge action of arsenic manifests itself more promptly in the tertian than in the quotidian variety of intermittent fever. He lays down rules for its administration, which are essentially the same as those prescribed by Boudin. He concludes from his observations that arsenic is as effectual, but not so quick, in its operation as quinine in the treatment of intermittent fever. On account of the slowness of its action and its tendency to aggravate the gastro-intestinal symptoms, arsenic is contraindicated in remittent fever.—*Arch. Gen. de Med.*, Sept. 1875. According to Kellner, arsenic increases the power of digesting fat, and decreases the amount of nitrogen given off in the urine, thus assisting materially in the formation of flesh.—*German Med. and Sci. Assoc.*, 1875.

ACIDUM BORACICUM.—In the *Indian Medical Gazette*, Surgeon-Major Watson writes that he has lately employed boracic acid in the treatment of vegetable parasitic cutaneous diseases with great success. In the different forms of tinea, and in that troublesome affection of the scrotum and inside of the upper part of the thighs to which Europeans in India are so liable, the local application of the acid acts like a charm. Dr Watson uses an aqueous solution of the strength of a drachm to the ounce, and with this he bathes the affected part twice daily, allowing the solution to dry on the part.

ACIDUM BROMHYDRICUM.—Dr D. C. Wade regards this acid as a powerful alterative, and a grateful refrigerant and sedative; he affirms that it increases the appetite and digestion, and acts as a general tonic.—*Peninsular Journal of Medicine.*

gallic acid. A teaspoonful of a saturated solution of the acid (containing about one-half grain to the teaspoonful) was given every two hours, and this was continued in one case for five days and nights in succession. Dr Jameson supposes that the gallic acid is carried by the blood unchanged directly to the congested and inflamed capillaries of the kidneys, acting as an astringent and tonic to them, promoting their contraction, and thus arresting the exudation of red blood-corpuscles, and promoting the normal secretion of urine. He has not seen the unpleasant effects from its administration which have been often referred to, but he thinks that if, in the commencement of the treatment, there be much fever with a coated tongue, it would do well prior to the exhibition of the gallic acid to act freely on the bowels by a brisk cathartic. Mr Sampson (*Lancet*, Dec. 1849) has recorded four cases of albuminuria, treated successfully by gallic acid, and the treatment has also been alluded to by Dr Wood and Prof. Stillé in their treatises on therapeutics.

ACIDUM SALICYLICUM.—Dr Winter extols the use of salicylic acid in the treatment of ulcers, whether recent or chronic; and Dr Wagner treats recent superficial gangrenous sores by applying to their surface a thin layer of the powdered acid, and covering it over with wadding; a week generally suffices to effect a cure. In atonic ulcers of the foot he found that the granulating process was accelerated by the use of an ointment made by rubbing up the acid with lard. According to Thiersch, salicylic acid when applied to wounds appears immediately in the urine (*Pharm. Centralhalle*, Oct. 22 and Nov. 5). In a recent number of the *Journal für Pracktische Chemie*, Wagner, amongst other matters, alludes to the extraordinary efficacy of salicylic acid in eczema of the head and face accompanied by discharge.

In a recent paper read by Mr Callender before the Clinical Society of London, while he admits the utility of salicylic acid as a local application to be fairly commendable, he states his opinion that it is inferior to other antiseptics, and unworthy of the strong recommendations which have been given to it. Prof. Salkouski believes benzoic acid to be superior to salicylic acid as an antiseptic—an opinion, however, which is not shared by Kolbe (*Berliner Klin. Wochenschrift*, No. 22, 1875). In a paper in the *Tennessee Pharmacal Gazette* for 8th Sept., Mr R. Rother writes, preferring benzoic

Zürn, and Fortheim, as to the efficacy of the acid as a disinfectant in the treatment of contagious diseases. Kolbe's statement, that *free* salicylic acid only is able to act as an antiseptic and disinfectant, and that salicylates have no such properties, may explain many apparent anomalies, and reconcile discrepant statements. In a case of dysentery, and one of diarrhœa, in which all remedies had failed, Dr Stephenides gave salicylic acid internally, and in the form of enema, with perfectly successful results.—*Wiener Med. Presse*, No. 14, 1875. Wagner bears testimony to the efficacy of the acid in all cases where fermentative changes occur in the contents of the alimentary canal.

Dr Fürbringer, in the *Centralblatt für die Med. Wissensch.*, No. 18, describes the results of experiments on the action of salicylic acid internally, and when given subcutaneously, in nine experiments on rabbits, in which septic fever had been artificially produced; the acid, in doses of from 0·05 to 0·2 gramme, produced distinct lowering of the temperature and rapid reduction of the fever. In the same journal Dr Butt states that he has found salicylic acid to be a valuable antipyretic in enteric fever, erysipelas, acute articular rheumatism, etc. Dr Moelé, of Rostock University Hospital, recently instituted some researches to determine the antipyretic value of salicylate of soda. In seven cases of typhoid, he found that the temperature fell from 2° to 3° C. after each dose of 4 or 5 grammes of the salt, and that there was also a marked diminution in the frequency of the pulse. In several of the cases sickness and vomiting obliged him to administer the salt as an enema; under these circumstances the fall in temperature was not so noticeable as when the salt was given by the mouth. Subcutaneous injections should always be made with a perfectly *neutral* salt.—*Berl. Klin. Wochensch.*

It has been observed that salicylic acid has a remarkable cumulative action, for after a first dose of from 4 to 8 grains, it has been found that a dose of one-half or one-fourth that quantity on the following days is sufficient to keep the temperature within good limits. Dr Langfeldt records a case of diphtheria in a girl two years of age, in which the obstruction to the larynx, which threatened life, was overcome by salicylic acid. Dr Fortheim has treated thirty-one cases of diphtheria with the acid without a single death.—*Memorabilien*, vol. 19. Dr Hanow also strongly recommends the use of salicylic acid in diphtheria; he uses a solution of one part of the acid, and one of phosphate of soda, in 300 of water, and of this he gives a tablespoonful for a dose to adults, and a teaspoonful to children.—*Berl. Klin. Wochens.*, No. 20. The effect of salicylic acid in diabetes mellitus has recently been tried in the Columbia Hospital Dispensary. The case treated was a typical one. The patient, a coloured woman, past middle age, had cataracts in both eyes, and there was at least fourteen per cent. of sugar in the urine. The salicylic acid was given in ten-grain doses in pill thrice daily, and the result of the treatment was, that the quantity of sugar was ultimately so reduced as to be incapable of being registered by Fehling's test, and the quantity of urine passed was brought down to the normal standard; all the other symptoms were also relieved.—*Boston Medical and Surgical Journal*, Sept.

In chronic cystitis the bladder has been washed out with a solution of salicylic acid, containing one part in five hundred of water; by this means not only was the odour of the urine removed, but it was also freed from pus. In empyema a solution of the same strength has been employed with very valuable results.—*New York Medical Journal*, June.

In an interesting paper communicated to the *Berlin. Klin. Wochenschrift*, No. 28, 12th July, Dr H. Bose states that the addition of borax greatly increases the solubility of salicylic acid,—so much so that 10 parts of the acid can be dissolved by the aid of heat in 100 parts of water, in which eight parts of borax had been previously dissolved. Dr Bose states that solutions for direct application to wounds should not exceed the strength of five per cent. of the acid. As a means of increasing the solubility of salicylic acid, Dr Squibb recommends phosphate of soda (one part to three of the acid); but Mr F. Toussaint (*American Chemist*) finds that phosphate of ammonia is more powerful in this respect than the soda-salt. He also finds that the acid is very soluble in diluted ammonia-water, or in a solution of carbonate of ammonia; and he suggests that, as salicylate of ammonia is more easily decomposed in the system than the corresponding salts of soda or potash, it may be preferable for medical use.

Wünderlich gives salicylic acid in an almond emulsion with syrup of almonds and orange-flower water, by which its taste is completely concealed.—*Wiener Med. Wochenschrift*, 10th April.

ACIDUM SULPHURICUM FOR BOILS.—Dr Madison Marsh regards sulphuric acid as almost a specific for boils and other analogous affections. To an adult he gives twenty drops of the acid in a glass of sweetened water thrice daily before meals, previously smearing the teeth with butter to protect them. After the boils have disappeared, he continues the administration of ten-drop doses of the acid for at least two weeks.—*Medical and Surgical Reporter*.

ACONITUM.—It would appear from Dr John Harley's observations (*St Thomas's Hospital Reports*, New Series, vol. v.) that aconite does not exercise that control over the febrile condition with which it has been accredited. If aconite possesses the febrifuge properties ascribed to it, its influence in this respect ought to be well marked in relapsing fever; yet in this disease it can in no degree anticipate or cut short the pyrexial stage. The following is a brief summary of the author's conclusions as to the physiological action of aconite :—As strychnia affects the whole of the cranio-spinal axis, so aconite affects a portion, producing an excitation which results in intermittent spasm. The chief sphere of action is the medulla, about the roots of the pneumogastric, hypoglossal, and spinal accessory, and from this point its influence radiates, diminishing in intensity as far forwards as the origin of third nerves, and as far down as the origin of the phrenic. Beyond these limits it exercises a depressing influence on the cranio-spinal axis, almost amounting to paralysis. Its action on the sensory function seems to be coextensive and coequal with its action on the motor functions. The sense of hearing is apparently wholly unaffected, and beyond the derangement of accommodation due to spasm or weakening of the muscles of the eye, the sense of sight is also unaffected. The sense of smell is not affected, but that of taste is greatly disturbed, both on account of its relation to common sensation and of the implication of the fifth nerve. Beyond the slight depression of function resulting in somnolency, aconite has no direct influence on the brain, and the effects produced by asphyxia have only a brief duration after a paroxysm, or immediately previous to death. The sympathetic nerve is not affected. At the moment of death the pupils dilate, and after death the heart may continue to pulsate; the heart is only secondarily affected, and is a consequence of the respiratory affection. Death is due to asphyxia and progressive collapse of the lung; the former being due to spasmodic closure of the respiratory passages and paralysis of the inspiratory muscles, and the latter being due to paralysis of the inspiratory muscles, and chiefly of the diaphragm. Schroff and Gubler seek to establish a distinction between aconite and aconitina, regarding the former as narcotic and acrid, and the latter as narcotic only; but M. Guillaud believes that their action is identical. By M. Leven aconitina is held to be a poison attacking simultaneously the central nervous system and the muscular system, and paralyzing both; but Guillaud has shown that convulsions are never absent during the first period of poisoning by aconite in healthy animals not anæmic from fasting, and even when the convulsions have ceased, the muscles long remain irritable to induced currents—facts which are opposed to the assumption that aconite is a muscular poison. Again, MM. Gréhaut and Aschscharumow consider it as a poison paralyzing the peripheric motor system; but M. Guillaud points out that, in addition to the circumstance that the paralysis of the motor nerves is only secondary, and that it only occurs like central paralysis after a period of hyperæsthesia, it is always consecutive to this last paralysis. The excitability of the nerve appears to die out from centre to periphery. Holtot and Liegeons originally, and Boehm and Wartmann more recently, maintained that aconitina is a poison which paralyzes the central nervous system, and this view is in accordance with M. Guillaud's observations. The latter author describes a series of phenomena occurring during the earlier stages of the action of aconite, viz., disturbance of locomotion, disordered movements, rapid and interrupted respiration, salivation, excessive secretion—all indicating hyperæsthesia of the nervous centres. Aconitina acts essentially on the centres of the spinal cord and medulla oblongata, by first augmenting their excito-motor functions, like strychnia and the salts of silver; it then gradually paralyzes them. With regard to the encephalon, the centres of voluntary movements are attacked and enfeebled, whilst the sensory centres remain, in part at least, uninjured. The drug then paralyzes successively the sensory nerves, the motor nerves, the sympathetic nerve, and then the muscles. It affects the respiration through the medulla oblongata, and not otherwise, and by this action causes death in the higher animals.—BROWN-SEQUARD'S *Archives du Physiologie*, August and September 1875.

According to Prof. C. D. von Schroff (*Zeitschr. d. Aster. Apoth. Ver.*, Nos. 19 and 20, 1875), aconite does not become innocuous when growing in particular localities, as has been stated, but the root and herb of some species of aconite are destitute of poisonous properties.

APOMORPHIA.—The following are among the conclusions summarized by Victor Bourgeois in his recent work, "De l'Apomorphin:"—It is a simple emetic, and does not appear to exert any influence on the other functions; it is innocuous, and does not seem to possess dangerous toxic properties. M. Bourgeois fixes as an average dose for an adult one-sixth grain hypodermically, or one half-grain by the mouth, of the apomorphia prepared by Messrs Macfarlan and Co., Edinburgh. The best form of solution is that obtained by dissolving one part of the salt in 100 parts of distilled water. If the proportion of salt be greater than this, the solution becomes muddy, and requires the addition of one or two drops of hydrochloric acid to clear it—a serious objection in the case of a liquid intended for subcutaneous injection.

According to investigations lately conducted by German physicians (*Allgemein. Wiener Med. Zeitung*, 1875), apomorphia is always best given subcutaneously, as it is not painful, and is not liable to give rise to abscess or dermatitis; its action is also more positive, and the required dose is less than when it is given by the mouth.

From Harnack's experiments (*Centralblatt für die Med. Wissensch.*, No. 2, 1875) it would appear that apomorphia acts on certain parts of certain animals in an intense and deleterious manner. In rabbits, which do not possess the faculty of vomiting, he found that 0·15 grain to 0·45 grain produced death, while doses of 0·075 to 0·15 grain threw the animals into a state of intense excitement for some hours. Larger doses than these caused first unsteadiness of gait, the posterior extremities being first affected; the animals then suddenly fell over in convulsions, the respiration ceased, and death ensued, and could not be averted by artificial respiration. The frequency of the respiration was at first greatly increased, owing, according to Harnack, to stimulation of the respiratory centre. This stimulation was followed by a diminution of the excitability, and ultimately by paralysis of the centre. He found that in chloralized animals convulsions do not occur, and he ascribes the absence of the convulsions to the paralysis by the chloral of these central organs, upon whose direct stimulation by apomorphia the convulsions depend. From his experiments on narcotized dogs he decides against the assumption that the respiratory and vomiting centres are identical. In the frog the author observed first a stimulation and then a complete paralysis of the centre of voluntary movement; later, the irritability of the transversely striped muscles is diminished, and with larger doses completely paralyzed, without their becoming rigid. This paralytic action on transversely striped muscles apomorphia possesses in common with several other emetic substances, *e.g.*, emetin, tartar emetic, cyclamin, and asclepiadin—a substance which, according to Harnack, is analogous to apomorphia. Lastly, he attempts to establish experimentally in what way the increase in the frequency of the pulse accompanying the vomiting arises—whether it owes its origin to a diminution in the tonus of the vagus, or is due to stimulation of the accelerating apparatus of the heart. Supported by the fact that stimulation of the accelerating nerves produces increase of the frequency of the pulse without increase of the blood-pressure, and that, on the contrary, diminution of the tonus of the vagus is accompanied by increase of the blood-pressure, he observed the pulse and blood-pressure after injection of apomorphia in dogs, and found that increase in the frequency of the pulse produced by the act of vomiting is not accompanied by an increase of the blood-pressure. Therefore, the relations existing during the act of vomiting are analogous to those which are produced by artificial stimulation of the accelerating nerves.

In the *New York Medical Record*, 7th August, Dr Duncan gives a paper on the hypodermic use of apomorphia as an emetic in children. He infers from his experience of it, that its value cannot be too highly estimated, and that it acts more rapidly and certainly, and as efficiently as the best of the older emetics. The average time at which emesis occurs is 2·9 minutes, and the longest time at which emesis occurred he met with in a case of alcoholism, in which vomiting did not ensue for 4·15 minutes. The rapidity of the action of apomorphia is a valuable property in the treatment of croup, or capillary bronchitis, and poison. The English preparation he prefers to the German, which is reputed to contain an impurity of morphia. The entire visible effect of the drug disappears after the lapse of an hour or an hour and a half, and the child's system does not appear to have been disturbed by it. The other effects than emesis, such as a tendency to sleep, yawning, slight weakness of the legs, etc., are very slight and transient. With regard to the dosage of apomorphia hypodermically, Dr Duncan states that for an adult it should range from $\frac{1}{10}$ to $\frac{1}{20}$ grain, but that in children it is large in proportion, thus:—

For a child,	18 months,	gr. $\frac{1}{50}$
„ „	2 years,	gr. $\frac{1}{40}$
„ „	3 „	gr. $\frac{1}{35}$
„ „	$3\frac{1}{2}$ „	gr. $\frac{1}{35}$
„ „	5 „	gr. $\frac{1}{30}$
„ „	8 „	gr. $\frac{1}{25}$

The following is the formula recommended by Dr Duncan—℞. apomorphia, gr. vii.; spiritus rectificati, ♏ xx.; glycerine, ♏ x.; aqua, ♏ l. M.

That the employment of apomorphia is *not* always unattended by inconvenience and even risk, is shown by cases recorded by Dr Perrèquet of Benzeville, and Dr Prevost of Geneva.—*Journ. de Méd. et de Chir.*, June 1875.

Part Fifth.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.—The following are the office-bearers for 1875–76:—*President*—Dr Gillespie. *Vice-Presidents*—Dr G. W. Balfour, Dr Littlejohn, Dr Keiller. *Councillors*—Dr Clouston, G. H. Gilruth, Esq., Dr Moinet, Dr P. A. Young, Dr Batty Tuke, Dr James Carmichael, Dr Hardie, Dr Affleck. *Treasurer*—Mr Joseph Bell. *Secretaries*—Dr Muirhead, Mr Chiene.

EDINBURGH OBSTETRICAL SOCIETY.—The following are the office-bearers for 1875–76:—*President*—Professor A. R. Simpson. *Vice-Presidents*—Dr James Young, Dr Alexander Milne. *Treasurer*—Dr William Craig. *Secretaries*—Dr Charles E. Underhill, Dr James Carmichael. *Members of Council*—The office-bearers, and Dr Angus Macdonald, Dr Robert Bruce, and Dr John Burn. *Librarian*—Mr J. Jamieson.

ROYAL MEDICAL SOCIETY OF EDINBURGH.—The following is the list of office-bearers of this Society for the session of 1875–76:—*Presidents*—A. R. Turnbull, M.B., C.M., A. R. Coldstream, M.B., C.M., L.R.C.S. Edin., Thomas R. Ronaldson, M.B., C.M., Thomas D. Wilson, M.A., M.B., L.R.C.S. Edin. *Hon. Secretaries*—George Ogilvie, B.Sc., Leslie Ogilvie, B.Sc. *Curator of Library*—J. J. Cox, M.B., C.M. *Curator of Museum*—J. Graham Brown, M.B., C.M. *Hon. Treasurer*—J. R. Young. *Sub-Librarian*—John Turner.

At the Academy of Medicine, during its meeting of the 30th of November 1875, M. Gosselin, President, the following list of Candidates for the honour of Foreign Associate (Membre associé étranger) was presented:—In the first line, Christison, of Edinburgh; in second, Donders, of Utrecht; in third, Pirogoff, of St Petersburg; in fourth, Tyndall, of London. The number of votes being 64, the absolute majority required 33. Christison obtained 46 votes, Donders 18;—in consequence of which Dr Christison (Sir Robert), was elected.—*Gazette Médicale de Paris*, 4th December 1875.

American Otological Society.—At the last meeting of this Society, held in Newport, R. I., 21st July 1875, it was voted—That the Committee on an International Congress be empowered by this Society to issue a call for an International Otological Congress, at such time and place as they shall see fit. In accordance with this vote, the Committee have called a Congress, to be held in New York City on Friday, 15th September 1876, at Ten o'clock A.M.; the place of the meeting to be announced later. Members of the medical profession who take an active interest in aural surgery, are cordially invited to be present and take part in the Congress.

D. B. St J. Roosa.
Clarence J. Blake.
Hermann Knapp.
J. Orne Green.

OBITUARY.

The late Robert Brown, M.R.C.S. Eng., F.R.C.S. Edin., of Carlisle.—Died at Banks House, Lanercost, Cumberland, on Friday, 19th November 1875, Robert Brown, surgeon, aged 43 years.

The announcement of the death of this very able surgeon cannot have taken his friends by surprise. It was well known by them, that after several months of failing health, Mr Brown had in June 1872 relinquished his practice in Carlisle, his connexion with the Cumberland Infirmary, and his partnership with Mr Page, the eminent surgeon of Carlisle.

While prepared for the termination of his sad illness in death, it may, however, be affirmed with a strict regard to truth, that the sorrow occasioned by Mr Brown's decease has been widespread and profound. His distinguished professional abilities, more particularly in the field of surgery, had acquired for Mr Brown the confidence of the public, not only in the city of Carlisle, but to a wide extent in the neighbouring districts of country; and with this reliance on his professional capacity, was associated a very large amount of esteem and love for the man. By those who were at all intimately acquainted with Mr Brown, the statement will not appear exaggerated, that in him there existed a rare union of such qualities of head and heart as necessarily commanded the admiration and regard of those with whom he was brought in contact. His relations with his medical brethren were always of the most pleasing description; and there are many, in the foremost ranks of the profession, who were wont to look upon Robert Brown as the impersonation of all that was admirable in the practitioner of the healing art.

A few notices of his useful career may not be uninteresting to our readers. The father of Mr Brown was a farmer living at Wintershields, on the borders of Bewcastle, a very wild part of Cumberland. There, Robert was born in 1832, and received his early training at Croft House, Brampton, at the period in question, and since, an excellent seminary of education. After leaving school, Brown was apprenticed to Dr Graham of Brampton. While living there during his apprenticeship, and on returning at the close of different sessions from his studies in Glasgow and Edinburgh, he was much noticed by the late distinguished physician of Guy's Hospital, Dr Addison, who was in the habit of frequently visiting the neighbourhood. At Lanercost, indeed, the honoured dust of Thomas Addison now lies. From Brampton he went to the Andersonian Institution in Glasgow in 1849 for one year, and thereafter entered as a student the University of Edinburgh.

In Edinburgh, nearly the whole of his professional education was conducted. He was a most diligent and successful student, acquiring the late Dr Penny's prize in Chemistry, and Professor Buchanan's for Anatomy, while in Glasgow; and in Edinburgh, the senior prize in the surgery class of the late Professor Miller. During the latter part of his residence in Edinburgh, Mr Brown was, after a keen competitive examination, elected assistant in the Pathological department of the Royal Infirmary, then under the direction of Dr Gairdner, now Professor Gairdner of Glasgow. This relationship led to the formation of a very warm friendship, never interrupted, between Mr Brown and a teacher of whom he was always ready to speak in terms of most affectionate regard. On subsequently leaving Edinburgh, Brown studied for a short period in London. After obtaining the Membership of the Royal College of Surgeons of England in 1853, he went to Carlisle, in the first instance as resident medical officer to the Dispensary there, and shortly afterwards became house surgeon to the Infirmary. Having by the admirable discharge of his duties in the latter position attracted the attention and gained the esteem of Mr Page, that gentleman, in 1857, invited Brown to join him in partnership. In this capacity, he entered upon practice in Carlisle, and in it he remained till the unhappy failure of health compelled him to relinquish all professional occupation. While this partnership lasted, there existed the greatest possible cordiality between Mr Page and Mr Brown; indeed, to many mutual friends, of whom the writer of this notice is one, it appeared to be at all times a partnership of uncommon happiness, as it certainly was one of very remarkable public usefulness.

Mr Brown's success in practice was very great. Mr Thom, of Brampton, a very attached friend, writes: "You know how he got on when once established. It is impossible that any one could have succeeded better. He was universally beloved, and had, I believe, no enemies whatever. Surgery was his favourite branch, and he was most successful in it. But, as I have often remarked, I never knew any one who was so well acquainted with all branches of practical medicine."

Mr Brown was elected Fellow of the Royal College of Surgeons of Edinburgh in 1865, and in 1857 had been appointed one of the surgeons to the Cumberland Infirmary. He was also Member of the Royal Medico-Chirurgical Society of London, and of the Cumberland and Westmoreland Antiquarian and Archæological Society. To the literature of his profession he was too much engaged in practice to make frequent contributions; but he published, in the *Lancet*, an interesting case of tracheotomy for a foreign body in the windpipe, and in the *Edinburgh Medical Journal*, a case of traumatic tetanus, successfully treated by large doses of bromide of potassium.

In practice Brown's work was laborious; and, unfortunately, he had many trials, particularly in the death of near and dear relations, which told in a very serious manner on his highly sensitive nature. The first symptoms, indeed, of the insidious brain disease, which led to death, showed themselves on the day of his sister's funeral. A warmly attached friend writes: "Grief occasioned by the loss of relations had, I believe, much to do with his fatal illness."

Looking back on the years of intimacy which the writer of this very inadequate tribute to his memory was privileged to enjoy with Robert Brown, he can truly say that a more affectionate and generous character, seeking its highest delight in the discharge of professional duty, he has never met. The medical profession was worthy of Robert Brown, and he was among its most worthy members.

"Quis desiderio sit pudor aut modus
Tam cari capitis?"

The following account of our dear departed friend, as he was seen in the discharge of his daily duties, cannot fail to be read with interest by many, even among those who had not the pleasure of his personal acquaintance. The writer was on terms of intimacy with the subject of our notice during many years, and had frequent opportunities of seeing him.

A true picture of Mr Brown's daily life, when he was in full practice, would give some idea of how intimately the healing art is mixed up with the life of the community. He gave himself so entirely up to his profession, and through it to the good of his fellow-men, that though not a "public man" in any conventional sense, he was so in a far truer and more complete way than most men who are so called. He began work every day about nine o'clock, when not up all the previous night, usually seeing his town patients and those near town first, and going to do his hospital work afterwards. This he took great delight in. Surgery of all kinds he undertook, and was most fond of. He seemed to have just three recreations. One was his hospital visit, another a chat and smoke with a friend in his own room in the small hours, and the third was to see a good horse. He appeared to enjoy equally a chat with his patients in the hospital, an important operation, a pathological inquiry in "Dick's" unsavoury den, or a gossip with the house-surgeon.

He returned to his house about one o'clock. This is the ordinary dinner-hour of the "burghers of Carlisle," and the consultation-hour of all the doctors. When, or how, Mr Brown dined was always a marvel to his friends, for when he returned at this hour he always had patients waiting, and he never kept any of them one moment on account of anything connected with himself. Humble town labourers, well-to-do tradespeople, country folks, and county gentry,—all went to him, and all seemed to look on him as a friend to whom they could come, not only for medical advice, but for counsel in regard to family matters and the difficulties of life. He seemed equally glad to see them all, and equally kind and attentive to each one's tale. His manner to his patients and to every one was quiet and unaffected; his pleasant smile cheered them, and his evident earnestness and capacity made them trust him fully. In any difficult or serious case, his own anxiety and sympathy were unbounded. He would go to see such a case many times a day, would sit up all night, and never be wearied by hearing the anxieties of relatives in regard to it. His throwing himself into his cases thus with mind and feelings was undoubtedly an exhausting process for him, but it was largely the secret of his being so largely beloved and trusted. When his health at last gave way, it was looked on in Carlisle as a public calamity.

Soon after two o'clock he started out, and saw his country patients, or met his country professional brethren in surgical consultation—a kind of practice of which he had a great deal. His manner as a consultant always showed much tact and knowledge of human nature. Without in any way hurting the feelings of his brethren, he always expressed and acted on his own opinion, after it was formed, in the most decided way. Indeed, he was a great favourite with the country practitioners in Cumberland, who trusted him implicitly.

Returning home late in the evening after those long and tiresome drives, often over rough roads, when most men would have rested, he had a cup of tea, and began his medical reading, very often going out to see some of his cases in the evening, or having to go for as long a drive as he had already had. Those calls were all too frequent, indeed, for his strength, but they were never put off for a moment. He entirely denied himself the relaxation and pleasure of formally dining out with his friends, or going into society; but no one ever made a friend more welcome, and how varied a circle he had! If only one was lucky enough to find him in—and the later the hour the more chance there was of this—he always made one welcome, never looked bored, and never appeared to grow tired of you. You might stay till one, or two, or three o'clock in the morning—it was all the same to him. Only one thing put an end to the meeting on his part, and that was a summons to see a patient, and that did so at once. Lying on his back on a peculiar sort of wide couch, smoking, with the latest medical book beside him, surrounded by two or three dogs of various sizes, he would discuss most keenly any question of professional interest, general news, or local politics. He had the most decided views on politics, but his warmest friends were many of them of the opposite party. Any one who knows how strong party feelings run in Carlisle, knows also how few men live above them and the petty social distinctions in the intercourse of life. Mr Brown certainly did so.

Wednesday, being market-day in the old border town, was his busiest day. His house would be full of country people all morning, and well he knew their ways. No wonder they were all so fond of him. But his house was never so full, even on a Wednesday, but that he would run out with a friend who could appreciate his points to show him the horse he had reared last. He always drove good horses, and it was a real pleasure to him to see them go. In the course of his life he had many most serious carriage and horse accidents, broken bones and bruises, but he was as ready as ever to the last to drive a good horse, no matter how high his mettle was. We can vividly recall his intense chagrin when, on one occasion, a young horse that he had sent up to the Islington Show, and whom he expected to get a prize, got hurt in the train.

The fees he charged his patients were absurdly small, but he never raised them, even when his practice was more than he could overtake.

DEATH OF M. GIRALDES.—Giraldes, one of the most industrious and erudite of French surgeons, and one of the very few of that nation who have thoroughly understood and appreciated British surgery, died in harness on 26th November, in the 67th year of his age. A week before his death, he had taken an active part in a discussion in the Society of Biology; and, three days before, he had also spoken at the Academy of Medicine. It was in the Library of the Faculty, just after leaving the Anatomical Society, that he was seized with the malady of which he died. Alphonse Guerin pronounced his éloge as representative of the Academy of Medicine, and Armand Moreau from the Biological Society. Both bore emphatic testimony to the zeal, industry, and talent of this great surgeon.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

EDINBURGH, 14*th December* 1875.

SIR,—Dr Littlejohn, in attempting to elude the proof of his inaccuracies in criminal law, as shown by my quotation from Alison, is now fain to admit that his authoritative, and, as he thought, crushing denial, in reply to my statement, that a charge of murder included a charge of culpable homicide, was only a quibble calculated to mislead no one but those more ignorant of criminal law and procedure than himself.

As he cannot combat Alison's dictum, which showed him to be at fault in his criminal law, he sneers at Alison. Shall I quote Hume to convict him? Such labour were vain! Dr Littlejohn values his own inward consciousness in matters of Jurisprudence far above all acknowledged law and authority. Surely he forgets that while such dogmatism may pass current for law in his class-room, it is not likely to impose on any one conversant with such subjects.

As you, Sir, have intimated that this correspondence must now terminate, let me, in conclusion, briefly review the points which in its course have been prominently brought forward on either side of the controversy.

The correspondence arose out of a statement made by me at a meeting of the Medico-Chirurgical Society, on the 5th May 1875, and reported in the June number of the Journal, at pages 1132–34. The passage referred to is as follows:—"From this experiment, then, as well as from a consideration of the reasons already given, he (Dr Watson) was able to show that the wound causing the death of the woman had not necessarily been inflicted intentionally by her husband, but might have been purely accidental, and this had led the prosecutor for the Crown to withdraw the charge of murder, and only press[1] that of culpable homicide. The knife with which the experiment was performed, and the ribs of the wounded subject, were shown."

Dr Littlejohn's original letter in your August number impugning my description of the preparation, and the important bearing it had on the result of a trial for murder, contained three inaccurate statements, to which I took exception. These statements were—1. That I experimented upon "a part of a thorax" with an "ordinary table-knife;" 2. That the effect of my experiment was only to corroborate the evidence of the medical witnesses for the Crown — that such an injury as killed the unfortunate woman *might possibly* occur from accidental causes; 3. That the plea of culpable homicide was preferred twice by the prisoner, and pressed upon the Crown by his counsel and agents. I showed in my letter of September, that these statements of Dr Littlejohn were inexact, in so far as (1) I experimented upon an *entire subject,* and with a *carving knife,* which corresponded in every respect with the actual instrument which inflicted the fatal injury; (2) My experiment proved to a demonstration, not the possibility of such an injury occurring from accidental causes, but that experimentally it was caused by a *fall,* as contra-distinguished from a *blow;* (3) That the suggestion of the plea of culpable homicide originated, not with the prisoner or his agents, but with the Advocate-Depute, who, after my evidence, expressed his readiness to depart from the graver charge of murder, and to accept instead a plea of culpable homicide.

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[1] Dr Littlejohn appears to attach some unusual meaning to the term "press," as employed by me in describing the action of the Advocate-Depute. The term is of course figuratively used, and is antithetical to the term "withdrew" used in the previous clause. There is no violence implied in the idea, as Dr Littlejohn seems to think. The sense is, that while he withdrew the major charge, he still preferred the minor charge of culpable homicide against the prisoner.

Dr Littlejohn ventured, further, in his first letter to state, that "an accidental death of the kind spoken of by Dr Watson involves no criminality." I took no notice of this palpable blunder at the time, taking it for granted that it had fallen from such a legal authority as Dr Littlejohn *per incuriam;* but as he reiterated it again in the October number of the Journal, and then ventured to found upon it the rash statement that I did not understand the meaning of the term "culpable homicide," I pointed out that his statements were inconsistent with the law on this matter, as laid down by Alison, and that, so far from my being ignorant and wrong, the ignorance and error were on his side.

Dr Littlejohn further stated argumentatively, that were my views of the importance of my evidence correct, the jury should have found a verdict of "not proven," and the prisoner have been discharged from the bar. I showed in my reply that the jury never had a chance of giving the prisoner the benefit of such a verdict, for after the suggestion from the Advocate-Depute that a plea of culpable homicide would be accepted by the Crown—and this was most unwillingly given by the prisoner—the presiding judge instructed the jury that they had nothing further to do than ratify that plea by finding him guilty of culpable homicide. My original statement, that my evidence led the prosecutor for the Crown to withdraw the charge of murder, and only press that of culpable homicide, was therefore shown to be consistent with the facts as they actually transpired.

If Dr Littlejohn's version of the story were correct—and it was not the excellence of the evidence for the defence which led the public prosecutor, after only that portion of the evidence had been given, which I imported into the case, to intimate his willingness to accept a plea of "Culpable homicide,"—I beg to ask again, Why did he depart from the original charge of murder, and accept the plea of culpable homicide at all? Especially, as Dr Littlejohn dogmatically informs us that "the evidence of the Crown was consistent throughout." Was it Dr Littlejohn's expressed opinion that the wound *might possibly* have been accidental, or could it be, that having led the authorities upon the ice, by very strong statements in his precognition, he suddenly became unwontedly cautious in the witness-box, when he saw tokens of evidence for the defence, of which he had never heard a whisper?

The unfortunate man was tried for murder is undoubted. That charge was departed from, after hearing a portion of the evidence for the defence, is undeniable. I suggested that such a result, taken in connexion with the evidence which was led, could only be attributed to one of two things—either the excellence of the evidence for the defence, or the total failure of the "consistent" evidence for the Crown. Dr Littlejohn had said it was not the former. I congratulated him upon the horn of the dilemma upon which he had transfixed himself. Let us see how he wriggles on its spike. Let us observe how he would reconcile the evidence adduced with the issue of the case. Let us see what theory he suggests to account for the result of the trial. The prisoner escaped from being found guilty of murder, and was condemned to fifteen years' penal servitude, not because the evidence for the Crown broke down; not because the exculpatory evidence was found sufficient to exonerate him from the major charge, but, according to Dr Littlejohn, because the prisoner was insane! This insanity theory is very mildly put at first in Dr Littlejohn's original letter, where he speaks of the prisoner as "of a low type of intelligence, of intemperate habits, and on the night of the murder as besotted with drink." Where also he speaks of knowing somehow that the judge and public prosecutor thought it possible that the prisoner at the time of inflicting the injury was beside himself with liquor, and that, therefore, he was permitted to escape from the capital charge. Surely a strange view of the state of the law and the administration of justice in this country to be taken by an expert in Jurisprudence! This view undergoes a modification in his letter in the October number of the Journal, where he says that "the sole difficulty in this case was as to the amount of his mental responsibility for the crime" (whatever that extraordinary phrase may signify). A supplementary allusion to some certificate of the prisoner's insanity, obtained from some medical men, might lead the unwary and those unaccustomed to Dr Littlejohn's usual method of argument, to suppose that this was forthcoming at the trial, while the allusion in his last letter to a perfect cloud of medical witnesses, as present in court, and ready to speak to his mental infirmity, seems almost to imply that in some way or other their evidence was before the court, and influenced the result. But all this insanity theory falls to the ground like a castle of cards when it comes to be told that no such evidence was ever adduced in the defence, and that this galaxy of alienistic learning was never unveiled. The case terminated as I have said, after my evidence as to the accidental nature of the injury, and this result justified me in the statement that the insanity theory had no influence in the disposal of the case. Were a prisoner in such circumstances saved from the gallows upon a plea of insanity, detention as a criminal lunatic during Her Majesty's pleasure would have been the result—not a sentence of fifteen years' penal servitude.

I have very carefully weighed all that Dr Littlejohn has brought forward in what he calls a "desultory"[1] manner in this correspondence, and have failed to be convinced that, in any particular, my original statement was incorrect. I have, I believe, conclusively answered all his arguments. I have shown his facts to be inexact, his conclusions rash, and his acquaintance with Criminal Jurisprudence sadly out at elbows. I am content to leave my statements alongside of Dr Littlejohn's without fear as to the verdict at which a jury of your readers would arrive—more especially as to the imprudence of this personal attack upon me in connexion with matters in which his name never appeared.

I see, however, we are not at an end of Dr Littlejohn's lucubrations on "this important case." I shall await with interest the lengthy allusion to it with which again he is to favour us "in the series of papers upon the Practice of Medical Jurisprudence which are being published in this Journal," in the confident hope that what has now transpired may not be without its effect, and may secure for these contributions a greater accuracy of statement, a greater attention to logical induction, and a less dexterous manipulation of ideas on the subject of criminal law and procedure than has been shown by him in these letters. If this be not so, I fancy we shall enjoy a treat in the very "original" character of these communications, in another and very different signification of that term than was intended in the advertisement which first heralded their publication to your readers. Should Dr Littlejohn, in these lengthy communications to the Journal, venture to open afresh this discussion, or, under the guise of a communication, make a fresh attack upon me, I shall deem myself entitled again to open up the argument, and claim from you the privilege in these pages of a reply.—I am, Sir, your obedient servant,

PATRICK HERON WATSON.

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[1] DESULTORY, *adj.*, jumping from one thing to another, *without a rational or logical connexion.*—Vide Chambers's Etymological Dictionary of the English Language.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Practice of Medical Jurisprudence.* By H. D. LITTLEJOHN, M.D., F.R.C.S. Ed., Surgeon of Police and Medical Officer of Health for Edinburgh; Lecturer on Medical Jurisprudence and Hygiene, School of Medicine, Edinburgh.

NO. III.—POST-MORTEM EXAMINATION.

THERE are two occasions on which a medical practitioner may be called upon by the authorities in Scotland to grant reports as to dead bodies, without instituting a dissection, or what is usually termed a post-mortem examination:—

1*st*. When a sudden death has occurred unattended by any suspicion, and the practitioner who is summoned is enabled from previous medical attendance upon the deceased to certify the cause of death, or when the mode of death (as in certain kinds of suicide and death from accidental injury) is apparent from a simple inspection of the body, a certificate stating the probable cause of death is considered sufficient by the authorities, and a post-mortem examination is dispensed with. The medical man may not, however, have seen the deceased before, and yet, from the suddenness of the death, the age of the deceased, and the symptoms spoken to by the friends, he may still be enabled, satisfactorily to himself, to certify the cause of death. This may be done in the following terms, and without insisting on a minute identification:—

Edinburgh, 15*th Dec.* 1875.

I hereby certify upon soul and conscience that I have this day examined, at No. 11 St David Street, the body of Thomas Jenkins.

Deceased was apparently about 60 years of age, stout and well formed. The body presented no marks of injury. The face was very pale; the features were calm and composed. On percussing the chest, the heart was found to be enlarged.

From this circumstance, and from the symptoms detailed to me by the wife of deceased, and also from the general appearance of the body, I am of opinion that deceased died from natural causes, in all probability from syncope depending on disease of the heart.

(Signed) ———

Such a report, accompanied by a detailed statement of evidence collected by the police, is in Scotland forwarded to the Crown. If considered satisfactory, no further proceedings are taken.

It ought, however, to be remembered that in such a case the medical man is entitled to insist on a partial post-mortem examination, such as will enable him to certify more certainly the cause of death. In the great majority of cases a simple inspection of the organs of the chest and abdomen will suffice to discover the exact cause of death. This is effected rapidly, without any disfigurement of the body, and the friends very rarely nowadays make any objection. Indeed, it is seldom that we do not find deceased insured in some life office, or a member of a burial society, and in these circumstances a medical man should insist upon a post-mortem examination whenever he is in doubt. In this case it is the interest of the relatives to grant every facility. Should they, however, obstinately object, the authorities must be appealed to. An officer will, in all probability, be sent to warn the friends that what the medical man proposes to do has already been sanctioned by the authorities, and that the body cannot be buried until the medical man is satisfied and can certify the death in due form. In the course of our experience we have had no difficulty in obtaining the permission of the friends to make the necessary examination.

2*d.* When he is summoned, say by a county constable, to inspect a dead body found on the public road or in any other unusual situation. Although ostensibly called in to certify the *fact* of death, in the great majority of cases the object of his being summoned by the constable is to speak as to the probable *cause* of death. The duty of the officer is, according to his instructions, to report the case in the first instance to his superior officer, who, in his turn, communicates with the public prosecutor. This official naturally wishes to have as complete information as possible, and it is obviously of importance that he be informed as to whether a simple inspection of the body is sufficient to enable a medical man to account for death, or if a more minute examination is not absolutely necessary to discover the cause of death. Under these circumstances a medical man may at any time be called upon to give a report of the external examination of a dead body, which may or may not exhibit marks of injury. The marks of violence may be such as to account for death, and at the same time may be of such an unequivocal character as to leave no doubt that deceased has been the victim of foul play. In these circumstances there can be little difficulty in stating the opinion that to clear up the case a more detailed examination is absolutely necessary, and such a report as follows may be granted :—

Edinburgh, 17*th December* 1874.

I hereby certify upon soul and conscience that, early this morning, 17th December 1874,
I was summoned by Police Constable John Stewart to inspect the body of a man (name unknown) lying by the side of the Glasgow road, about a hundred yards to the east of Clermiston Toll.

Deceased had been subjected to great violence. Among other injuries there is a penetrating wound of the left breast, from which much blood has escaped. The scalp is also severely wounded, and the skull is evidently fractured.

I am clearly of opinion that a post-mortem examination should be made without delay. (Signed) ———

On the other hand, where there are no external marks of violence, or these are comparatively slight, and not such as to compromise life, the middle paragraph of the report may then read as follows:—

"The body presented no marks of violence, and nothing to indicate the cause of death." Or,—

"With the exception of a few trifling bruises on the head and lower extremities, the body presented no marks of external violence.

"I am unable to account for death without a post-mortem examination." (Signed) ———

Armed with such a document, the constable proceeds with his report to headquarters, at the nearest county town, and the public prosecutor at once proceeds to investigate the case. In our experience medical men have often shown an unwillingness to grant such preliminary reports, on the ground that they are not ordered by the authorities, but are merely requested by a constable. There should be no hesitation in the matter. Without some such information from a medical man, the public prosecutor might, in some of the cases alluded to above, delay ordering a post-mortem examination until more complete inquiries are instituted; and, annoyed at the reticence of the medical man first summoned, he might overlook his claims and order the examination to be conducted by others. On the other hand, when such a report is presented to the authorities the responsibility of the case rests with them. The medical man has not only done his duty as an ordinary citizen in a grave emergency, but he has also given a decided opinion as to an important step which should be taken for the proper investigation of the case. Should his advice not be followed, and a miscarriage of justice take place in consequence, his report speaks for itself and exonerates him from all blame. The authorities, however, are too glad to receive reliable advice in such circumstances, and, as a rule, the public prosecutors at once act upon these reports and order a post-mortem examination.

In any case of death, during the investigation of which it is possible that a person may be judicially examined regarding it, it is the invariable rule in Scotland to associate two medical men in the performance of the post-mortem examination. The medical man who has been summoned by the police on the discovery of the body, and who has furnished the preliminary report alluded to above, is named as one of the inspectors, and, as stated in the previous paper, another medical man living probably in the county town, and who has gained experience in similar cases, is generally conjoined with him. To give those gentlemen the necessary authority to make the examination they are furnished with a warrant, a copy of which is given below, and which it will be observed consists of a petition by the public prosecutor, addressed to the magistrate, setting forth the grounds of his application, and craving warrant to the inspectors to make the necessary examination—followed by the order of the magistrate granting the warrant:—

"*Edinburgh, 4th January* 1876.—Unto the Honourable the Sheriff of the County of Edinburgh, or his Substitutes, the petition of R. L. S., Procurator-Fiscal of Court for the public interest; humbly sheweth, that the petitioner has received information that this morning, 4th January 1876, A. B., wife of D. C., a grocer and spirit-dealer in Bread Street, Edinburgh, was found lying on the floor of her husband's house dead, and under suspicious circumstances. That in order to ascertain the cause of her death this application is necessary. May it therefore please your Lordship to take precognition respecting the premises, and to grant warrant to officers of court to cite persons to attend for that purpose; and to search for, and take possession of, all articles that may afford evidence in the said investigation; and, for that purpose, to make patent all shut and lockfast places; and, further, to grant warrant to Drs H. D. L. and D. W. to make a post-mortem examination of the body of the said A. B. and report, and to do further or otherwise as to your Lordship may seem meet. According to justice, etc. Signed, R. L. S., Procurator-Fiscal. *Edinburgh, 4th January* 1876.—The Sheriff, having considered this petition, grants warrant as craved. F. H."

Such a document is absolutely necessary, and in important cases, or where the slightest opposition on the part of relatives or interested parties is apprehended, should never be dispensed with. We have on several occasions been put to inconvenience and delay from not being provided with a warrant. In a case which occurred in the neighbourhood of Edinburgh, and where a husband was suspected of being accessory to the death of his wife, he positively refused to allow the examination to be proceeded with until a warrant was produced. It was in vain that the officers who accompanied us assured him that it had been left by mistake in the office, etc., etc., the man stood upon his rights as a citizen and defied us to touch the body of his wife until he was satisfied that we were acting under due authority. There was nothing for it but to despatch a messenger for the forgotten document, and after the delay of a couple of hours it arrived, was duly perused, and permission was then granted to the medical men to proceed with their task.

Furnished with a warrant, medical men are empowered to take complete possession of a body, to make as searching an examination as they deem necessary, and to remove any portion or portions of the body which they may judge of importance to examine with greater care, and possibly after the application of various reagents. It is no excuse to a medical man for the imperfection of his evidence that his examination was hurried in consequence, let us suppose, of the late period of the day when it was commenced, or because a pressing engagement obliged him to go to some other part of the country. The answer to that is twofold: 1st, that where the life of a prisoner is at stake, the medical man must sacrifice any other engagement, however pressing, to the important work he has in hand; and, 2d, that as the body is lawfully in his possession, he can, consistently with the special circumstances of the case, begin the examination at a time best suited for himself, taking care that the witnesses for the identification have had an opportunity as early as possible of seeing the body in the presence of the two medical inspectors.

Any one obstructing medical men acting under a warrant is liable to severe punishment, and officers of police will not hesitate to take into custody all persons who threaten to thwart or molest gentlemen so engaged. In our time no such case of obstruction has occurred, but in the experience of former surgeons of police it has happened again and again, particularly in the case of the Irish, when a death occurred in one of their faction-fights, that the medical inspector had to be escorted to the place of dissection by a strong body of police; and, on one occasion, owing to the smallness of their escort, they were actually besieged for some time until relieved by a stronger force. But these days are happily gone, and the majesty of the law is now duly respected. In all our large towns mortuaries or dead-houses are provided, to which the body can be removed, where the dissection can be made at leisure and with every appliance necessary for its satisfactory completion. A post-mortem examination having been ordered by the authorities, a sheriff's officer makes the necessary preparations at the place of dissection, takes charge of the witnesses who are summoned for the identification of the deceased, introduces them in turn, and remains in attendance to await the instructions of the inspectors. It may happen that the result of the examination is to show that a great crime has been committed, and it may be necessary on the spot to take decisive measures. On two occasions in our experience such an emergency has occurred. Once in Edinburgh, while conducting what appeared to be an ordinary dissection of an old lady who was reported to have died from bronchitis, a cork from a quart bottle was found firmly fixed in the upper strait of the windpipe; and as it appeared to the inspectors to be impossible for this foreign body to have got into its unusual position by accident, it was evident that murder by suffocation had been practised. A hint was quietly given to the detective officer in attendance, who left, and in a short time returned with a sufficient force and took all the inmates of the house into custody to await investigation by the authorities. A relative of the deceased, in the house at the time, was subsequently tried for the murder. The other case occurred in the country, and some miles distant from Edinburgh. A dissection had been delayed till late in the afternoon of a winter's day, in consequence of the unavoidable absence of the country doctor who had given intimation of the death to the police. He had refused to certify the death to the registrar, owing to the woman being moribund when he was called to see her. Otherwise, there was no suspicion beyond the suddenness of the death of a middle-aged female. In the course of the dissection, it was discovered that there was a punctured wound of the vagina, and corresponding apertures in the liver and diaphragm. It was at once apparent that a blunt instrument of considerable length, and of the dimensions of an ordinary walking-stick, had been forcibly inserted into the body, and had produced death by the shock caused by the severity of the injuries. No other cause of death was detected, and the case was evidently one of murder. Suspicion pointed to the other inmate of the house, viz., the husband, who, during the dissection, had occupied an adjoining room, and had amused himself with drinking toasts in the presence of a county constable. It was explained to the husband that it was necessary he should accompany us into Edinburgh, until the death of his wife was satisfactorily cleared up. We were prepared for resistance, but he assented to go with us, and that night he was lodged in prison. He was ultimately tried for the murder of his wife, and was found guilty.

The medical inspectors arrange among themselves who is to conduct the dissection, and who is to take notes. And on the principle of "*qui facit per alium facit per se*," each is responsible for what the other does. We knew of a case where an extra-conscientious inspector, not having observed an unlucky incision which his fellow had made by accident in one of the abdominal viscera, insisted on describing this so-called injury in the body of the report. The point so disputed was referred to the public prosecutor, who at once decided, on the rules of common sense, that the incision having been made accidentally by one of the inspectors, no mention of it could be introduced into the joint report. Of course, the dissection is narrowly watched by both, and any unusual circumstance attracting the attention of the operator, he at once points it out to the other inspector, taking care not to alter its position and appearance by any hasty incisions. This may happen to be the most important point in the dissection, and may determine the subsequent complexion of the case; and it is obviously of the greatest possible importance that the most perfect understanding should exist between the inspectors as to the nature and value of the lesion.

An important question here presents itself, whether the inspectors are permitted to have any assistance in the dissection, and whether any other medical man, in addition to those named in the warrant, may be present. An assistant—say, a medical student—may be employed to assist, but he must be a person of trust, who will not divulge the private talk of the inspectors, or the result of their deliberations. It must be remembered that this assistant may be summoned to give evidence for the defence. He may choose to differ from the finding of the report as to some minor point, or he may abuse the confidence reposed in him, and detail in the witness-box some of the doubts which may have escaped from the lips of the inspectors during their consultation. We confess that, in our experience, such a *contretemps* has never occurred; but it is better for the inspectors to do their work themselves, and thus avoid the possibility of such an annoyance.

The presence of other medical men, attracted by mere curiosity, is a more important matter. Those, from their experience and qualifications, are fully capable of forming an opinion, and are entitled to express it, more especially when its publication as evidence may tend to exculpate a prisoner. The question, however, is, should they have had an opportunity of forming an opinion on the subject at all? Now, the practice of our criminal law is explicit upon the point. Two medical men are named as inspectors in the warrant, and to them alone is entrusted by the Crown the task of making a thorough examination, and they are bound to report to the Crown *quam primum* the result of their investigation in the form of a report, which otherwise in the meantime is a sealed and secret document. No other medical men should be present. We remember, in a case that was tried before our highest criminal tribunal, one of the Crown medical witnesses, on being pressed to explain the grounds of a positive assertion as to the causation of an injury, forgot himself for the moment, and added that the opinion he had given was not only his own, but it was also that of a distinguished member of the medical profession who had been present at the dissection. The presiding Judge at once inquired of the Crown lawyers whether the distinguished person referred to by the witness was to be produced by the Crown, and on learning that he was not, the Judge very properly told the witness that it was irregular to have any other medical men present at the dissection than those named in the warrant, and that if he, the witness, felt himself not qualified for conducting such dissections, the sooner he retired from the post of medical inspector the better. We have several times been requested to allow medical friends to be present on such occasions, but have always refused. The grounds of our refusal have been frankly stated, and they have always been accepted as satisfactory. In our experience, now extending over twenty-five years, no application has ever been made on behalf of a prisoner that he should be represented by a medical man at the dissection. No higher compliment could be paid to our criminal procedure and the justice of its provisions. As we have already stated, in all strictly medico-legal dissections, one of the medical inspectors is deputed by the Crown to assist at the dissection, and his instructions are—1*st*, To see that the examination be as complete and searching as possible, and that no omission be made; 2*d*, That everything tending to exculpate the prisoner should be fully and frankly stated; and, 3*d*, That the report should be drawn up in due form. No doubt, subsequently on the trial, difference of opinion may exist as to the deductions drawn by the medical examiners from the facts observed by them, but rarely or never has the description of the facts themselves been called in question as inaccurate. My friend Professor Ogston, of Aberdeen, is in the habit of making no objection to the presence of medical men at the dissection who may be interested in the case. But he informs me that he takes the precaution of having a careful description of the appearances met with drawn up on the spot, and he insists on reading this in the presence of all the medical men, and requesting them to append their signatures in proof that the document faithfully represents the facts ascertained in the dissection. The inferences drawn from the facts are carefully considered in private, and are stated in the conclusion of the official report to be drawn up by himself and the gentleman associated with him. Of course, all the other medical men present at the dissection may appear and give evidence for the defence; and they are not likely to gainsay the facts ascertained at the examination, inasmuch as they have already certified to their correctness. But these signed notes are not produced on the trial, and no satisfactory reference can be made to them. Such a procedure certainly lessens the difficulties attendant on the presence of strangers; but it gives a certain number of medical men a *locus standi*, and an apparent right to give evidence in the case. Everything depends on the character of these witnesses of the dissection; and should one or more dispute the accuracy of the statement of the facts observed, the dissection might be indefinitely prolonged, and possibly with the unsatisfactory result of a permanent difference of opinion on points of great importance, leading to a possible miscarriage of justice. In England, on the other hand, in most important cases, only one medical man is employed at the dissection, and on his single testimony are the medical facts brought before the jury. This procedure is unsatisfactory, and necessarily exposes the prisoner to considerable risk. He has not the satisfaction of hearing any corroboration of the existence of unusual appearances, and of the opinions founded on them. His counsel has no opportunity of contrasting the evidence of two intelligent medical men, or of ascertaining, what may be of vital importance to his client, whether certain appearances spoken to by both are similarly estimated by them as of equal importance. As has been well said, "one's conviction gains infinitely the moment another will believe in it;" it is equally true that a fact clearly observed by two independent observers acquires an importance which no uncorroborated testimony could give it, and that the most experienced inspector has a feeling of satisfaction when he has the assistance and co-operation of a medical brother, to whom he can point out what has attracted his more perfectly trained senses, and who can corroborate his evidence.

A. *External Examination.*

The chief medico-legal writers have laid it down that the first duty of the medical inspector is to institute a careful external examination of the body. This is so far correct; but we must add the caution that, after examining the head, face, and the front of the body generally, the inspection of the back should be reserved to a later stage of the dissection. In a large proportion of cases of sudden death, more especially where the dissection has been somewhat delayed, when the body is turned on its face, a quantity of fluid escapes from the mouth and nostrils, and it necessarily happens when the body is replaced on the back, that this fluid gravitates backwards and enters the windpipe. It may come to be an important question when the air-passages are examined, how much of the fluid found in them may not have been thus accidentally introduced post mortem. We therefore regard the rule as a good one which limits, in the first instance, the external examination to the front or upper portion of the body. Of course any appearances of injury, etc., etc., present on the back, are not altered by the slight delay.

In this preliminary examination, the following points are attended to. The condition of the scalp as to bruises and wounds, the state of the face as to lividity or pallor, and the general expression of the features; the appearance of the eyes; the presence or absence of blood (fluid or dry) in the nostrils; the position of the lips, whether closed or open; their condition as to pallor or lividity; and the position of the tongue, whether protruding from between the lips, held firmly between the teeth, or applied closely against the dental arches; the front of the neck, for the presence of the mark of any ligature, or indeed for the slightest injury, such as the ruffling of the scarf-skin over however limited an extent. Any marks on the chest, abdomen, or extremities (other than the greenish hue on the lower part of the abdomen indicating the advance of putrefaction) should be incised so as to ascertain their true nature. The state of the hands and feet is to be noted, and generally the temperature of the body, and the presence of post-mortem lividity—of rigidity and putrefaction. Of course any wound is examined, measured, and described.

A very few minutes suffice for an experienced inspector to note all these points, and he proceeds to open the body. This is effected by making an incision from the top of the sternum to the pubes. Care should be taken, if the knife is sharp, that none of the more superficial abdominal viscera are injured, thus allowing of the escape of gas, and possibly of their fluid contents. This is at all times a disagreeable and awkward result, and in a case of suspected poisoning is greatly to be regretted. We have always been in the habit of deepening the incision about a couple of inches below the umbilicus, and at once entering the peritoneal cavity. The forefinger of the left hand is now introduced, and the walls of the abdomen are forcibly pulled upwards, and the knife is rapidly carried forwards, following the finger (which continues to elevate the walls) until the lower portion of the sternum is reached. By this simple procedure, we avoid injuring the intestines, the liver, and the stomach. In all cases of death from asphyxia the liver is much congested, and should it be accidentally wounded, a large quantity of dark venous blood escapes, obscuring the dissection. Again, in the great majority of cases of sudden death, the stomach is greatly distended with gas, and were the incision carried from above downwards with considerable force, this organ could not fail to be injured—to collapse—and, did it contain an ordinary meal, a portion of its contents would escape into the cavity of the abdomen. This is annoying, and in a case of suspected poisoning, the accident might prove to be of importance.

Much may be learned in the external examination as to age, height, state of health, mode of living, occupation, presence of deformities, and former injuries, and possibly as to the probable cause of death ; but in this preliminary incision two points cannot escape notice, and these are—1*st*, the condition of the blood as to fluidity ; and, 2*dly*, indications as to the habits of deceased. In all cases of death by asphyxia and of sudden death generally, the blood is fluid, and when an incision is made, say, in the skin of the chest, the edges of the incision are seen to present numerous bleeding points, and the blood issuing from them is noted to be venous. Whenever this appearance is seen, it may be taken for granted that the blood throughout the body is fluid, and some notion is thus gained as to the probable cause of death. Certain precautions, too, become necessary in the subsequent management of the dissection. Again, in the case of confirmed drunkards, the incision thus made has a peculiar greasy unctuous feel—and the fingers become slippery from the oil which has exuded. This, in our experience, is a certain sign of a life of intemperance due to the immoderate use of spirituous liquors. An important hint is thus obtained as to the organs likely to be found diseased, and also as to the probable cause of death—it may be direct intoxication, accidental suffocation when unconscious from drink, or, lastly, syncope from fatty degeneration of the heart.

(*To be continued.*)

ARTICLE II.—*Against the Pendulum Movement in Working the Midwifery Forceps.* By J. MATTHEWS DUNCAN, M.D.

(*Read before the Obstetrical Society of Edinburgh*, 12*th January.*)

MY object in writing this short paper is to contribute to the conclusive giving up of a bad practice. I have long publicly taught that the pendulum movement is useless and injurious; but that kind of publicity is not so wide as is desirable. Others, of great authority, from their recognised science and experience, such as Litzmann of Kiel, have made objection to this manœuvre, but it is still extensively used and vigorously recommended. There is, indeed, a natural tendency to resort to this swinging motion in the hands or minds of many practitioners, who probably have erroneous notions of what is called gomphosis, and think of drawing a nail out of a board. Besides, the movement is an element in that fussiness which recommends itself to many, if not the majority, of practitioners, and satisfies the admiring onlookers. The solemn arguments by which obstetric authors support its use I shall presently discuss.

My objection does not extend to changes in the direction of traction by the forceps, such as may be required according as the dragged head descends, or such as may be called for when the head has been previously inefficiently dragged in a wrong direction. It is to the changes implied in the oscillatory or leverage or pendulum-like movement of the dragging instrument that my objections are raised. These movements I regard as not merely useless, but as also injurious. They have been considered as leading to economy of force; but this is the opposite of the truth, as I shall hereinafter show. I mention this imagined advantage now, in order to say that, were it true only so far as the practitioner is concerned, I would esteem such economy as of no value, for the accoucheur is present with a practically boundless supply of force, which he is willing to lavish for behoof of his patient.

In the following remarks I shall make reference only to that pendulum movement from side to side, which alone is, so far as I know, recommended in these latter times. The pendulum movement in a sagittal direction, as recommended by the early describers of the forceps operation, is still more open to objections than the former.

In describing or defending the pendulum movement, two great points are made: first, that it is analogous to, or identical with, that of a lever and double rack; and, second, that by resorting to it there is an economy of force.

The lever and double rack hypothesis may be considered first. It is difficult to dispose of it only because it is so flimsy that it is impossible to get hold of it. There is no toothed rack on the wall of the pelvis. There is no roughness to take the place of a toothed rack. Were there any such teeth or roughness, the worst use that could be made of them would be to make them assume the function of a rack. The accoucheur should strive to advance the head as smoothly and with as little impression as possible on the walls of the pelvis. Further, there are no teeth or roughnesses on the fœtal head to fit into the teeth of the supposed rack.

Without the lever and double rack hypothesis substantiated, the movement must be useless; for it is, and must be, done to no purpose. It is conceivable that the head may be seized with such a degree of firmness by the blades of the forceps as to be moved or made to revolve as on a pivot; but such movement, without the additional force required in simple and successful traction, uncomplicated by the movement, would be of no avail. It would not advance the head. Without the lever and double rack, it would only cause revolutions as on a pivot—it would not produce progress.

Let us imagine that the accoucheur, dragging to one side, produces advance of the opposite side, which he maintains by continued dragging, while he makes the oscillatory movement in order to drag to the other side. In this way he may make the head advance while using an oscillatory movement. Every one knows that this is easily done. But then every one must also recognise the utter inutility of the movement. It is the imitation of the action of a lever and double rack without a trace of its utility. An unnecessary movement like this has great disadvantages. The pressure exerted, and the traction force used, are probably greater, certainly not less, than if simple traction were exerted to produce the desired result. Pulling the head down at one side and then at the other, and so advancing, is merely an injuriously complicated way of producing simple progress. It produces no evasion nor diminution of the difficulty to be overcome, while it has concomitant and avoidable evils.

In answer to this reasoning one might refer to an analogy in the way in which a cork is sometimes extracted from a bottle without using a corkscrew. But a study of this analogy only confirms the argument. For the cork would be better, and on the whole easier, extracted by simple uncomplicated traction as by a corkscrew. Besides, the cork and the bottle mutually exert such friction force as prevents retraction of one side if advanced, just as if the bottle were a rack and the cork the lever; and such is in no sense the case with the fœtal head and pelvis. Lastly, this oscillatory advance of the cork is only sought when power fails to produce a direct advance, and, in the case of the forceps, there is never deficiency of power; for, as we have already said, the power applicable by the accoucheur is practically boundless, or, in more sober terms, it is greater than he can safely apply.

The idea that there is any saving of force, so far as pressure on the mother's and child's parts is concerned, by resort to the oscillatory or pendulum movement, is such that I cannot argue against it. The question involved is purely mechanical and of extreme simplicity. It is this: a mechanical difficulty in bringing a child's head through a resisting passage has to be overcome; further, the difficulty is not to be evaded by changing the position of the child's head; on the contrary, that position may be supposed to be the most favourable for facility of propulsion. Now, can any oscillation or other imaginable movement diminish the mutual, and, in this case, injurious pressure or force required to produce advance? The question requires no answer. The supposition is absurd. Moreover, the absurdity is not less, whatever phase may be given to the hypothetical advantages of the pendulum movement. A certain amount of work has to be done; the head has to be advanced against resistance that must be overpowered if the effort is successful. Direct uncomplicated traction does the work in the simplest way, and no complication of it by pendulum movement or other can diminish the amount of work expended below that required by simple traction. The complication of simple traction may, however, increase the expenditure of force to a great extent.

The pendulum movement necessarily involves an injurious amount of pressure and consequent friction, in all cases, between the parts of the head to which the blades of the forceps are applied and the adjacent maternal structures. No doubt this friction is in most cases so slight and temporary as to be of little moment. But in some cases, when the resistance to progress arises from tight and undilatable soft parts, it may be very injurious. The most important forceps cases, however, are those where the obstacle to progress arises from hard parts; and, of these cases, the most frequent and characteristic are those of simple narrow or flat pelvis. In such cases the head has to be slowly dragged and perhaps moulded between the promontory of the sacrum and the pubic bones. Now, here, the pendulum movement involves special evils and dangers; for by it there is necessarily produced, besides the trivial friction, which is most extensive at the points where the blades are applied, a violent and powerful squeezing of the soft parts between the head and the opposing pelvic bones, on which the head works. This combination of wriggling and squeezing is altogether unnecessary, and must greatly aggravate the necessary or unavoidable mutual pressure, which is bad enough.

If, for the carrying out of the plan of pendulum movement, the forceps is made to compress the head so strongly as not to slip on it, which mode is probably regarded as desirable, then the points of the forceps, and especially the point of that blade which is on the side of the head towards which the movement is given, will exert a specially powerful, and certainly undesirable, amount of pressure on the parts of the child's head or face which they touch. If, on the other hand, the blades do not press the head with such firmness as to obviate a to-and-fro motion of them on the head, then the scalp will be liable to be much injured, and its surface abraded; conditions which are often observed as the result of this kind of proceeding,

There is, in the mechanism of delivery, whether natural or morbid, nothing analogous to this artificially produced oscillating or pendulum movement. Nature pushes a fœtal head through an obstructing passage by force, which produces, or may produce, on the one hand, dilatation or laceration of the passage; and, on the other hand, various kinds of changes in the shape and size of the fœtal head. Our best guide in forceps cases is the process of nature; and it is probable that any future improvements in the working of the instrument will be the consequence of closer and successful investigation of the mechanism of labour in these difficult cases. The pendulum movement, in working the forceps, does not advantageously increase the power of the instrument to produce desired results.

The use of the forceps is to contribute, by artificial pulling, to the strength of the natural expulsive efforts which push. To this traction, judiciously applied, the practitioner should confine himself. The oscillatory movement will contribute nothing to the forward traction, and it is the forward traction which alone is desirable.

In corroboration of these theoretical views as to the injuriousness and inutility of the pendulum movement in the working of the midwifery forceps, I might appeal to the extensive experience of myself, and of many other practitioners. But such appeal can only be held as evidence sufficient to show that the pendulum movement is not necessary. It affords no evidence that using it or abstaining from it is the preferable plan. And I cannot imagine any method available at present whereby the results of experience can be made suitable for the final settlement of the matter.

Both plans are used by good practitioners. Traction without oscillation is simple, effective, and in accordance with the method of nature's own efforts. Traction with oscillation is complicated; and many theoretical and practical objections may be made to it.

Other objections might be adduced against the pendulum movement, and some are candidly stated by authors who recommend its adoption. I have confined myself to the discussion of its supposed utility and inevitable evils. The only advantage which I can conceive it to possess, is one to which no one will avow his indebtedness, for it would be an admission of culpable want of knowledge, and consequent unjustifiable practice. A practitioner ignorant as to the proper direction of pulling may, by this motion of the forceps while extracting, fall accidentlly upon the right direction, and thus do some good by mere luck, and at much risk, which should have been done intelligently, and without avoidable risk. It is, perhaps, in order to ensure this kind of possible success, that some authors recommend the movement to be not pendulum-like, but rotatory, or in the grand so-called tours.

Article III.—*On Inflammation.* By G. Thin, M.D., London.

(*Continued from page* 616.)

Chapter III.—*Continued.*

Spindle-Cells and Colourless Blood-Corpuscles.

Fig. 30 represents the stellate, spindle, and colourless blood-cells in an osmic preparation of a seven days inflamed rabbit's cornea.

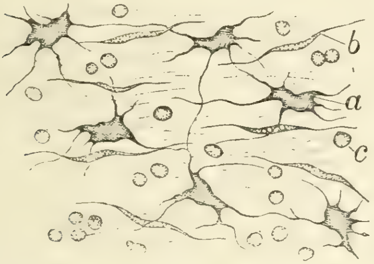

Fig. 30.—From a section of a rabbit's cornea seven days inflamed by thread. Osmic acid. Mag. 400 diam.
a, Stellate cells.
b, Spindle-cells.
c, Colourless blood-corpuscles.
(There being no further staining by logwood or aniline, the nuclei are not brought out.)

I have hitherto discussed the appearances of the spindle-cells as seen by methods which may be considered as comparatively perfect. There are other methods, in which the results are always imperfect, and have been consequently a fertile source of confusion and controversy. This is notably the case with gold preparations. If an inflamed cornea is placed in gold solution at that stage of the process in which the spindle-cells in osmic-acid preparations show granular protoplasm around and beyond the nucleus, and is then allowed to stain in acidulated water, there will frequently be seen in the cornea a number of oblong-shaped bodies arranged in parallel rows, and the rows in one plane frequently cross those above or below at a right angle.

Although these bodies are arranged in rows, they appear perfectly isolated from each other. They are further seen to be constituted by several small sharply-contoured, somewhat circular bodies, enclosed by a border of a very dark finely granular substance.

If the cornea or section is stained in logwood, the small central bodies are seen to be of the nature of nuclei, and to be arranged longitudinally in groups of two to four, corresponding in every respect with the spindle nuclei seen in osmic-acid preparations. Very frequently, nothing is seen but the nuclei, and they can be traced as following straight lines for considerable distances in the transverse planes of the corneal substance, the large undivided stellate nuclei being seen in the same tracts.

His first described this appearance, and it has since been seen and figured by many investigators. By His and others it has been ascribed to a proliferation of stellate cells; by Cohnheim, Axel Key and Wallis, and others, to in-wandered colourless blood-corpuscles. They constitute the "spiessartige Figuren" and colourless corpuscles in "spindel-form" of these different authors.

It is unnecessary to point out the objections that can be taken to either view. The demonstration of spindle-cells in the normal cornea, which have, as regards their relation to the corneal substance and to each other, the same position which these much discussed elements have, and the results obtained by osmic acid in the inflamed cornea, show that they are the nuclei and protoplasm of the inflamed spindle-cells. The processes of the cells are by this method invisible, but exist none the less.

The *spiessartige Figuren*, and the colourless corpuscles in *spindel-form*, are the inflamed spindle-cells of the normal tissue.

When a tissue containing bloodvessels is treated by osmic acid, the red blood-corpuscles are stained and "fixed" in a very characteristic way, on which I need not enlarge here. Suffice it to remark, that in such preparations they can be diagnosed with great accuracy. In a cornea in which a subacute inflammation has existed for seven days or more, and in which new bloodvessels begin to be formed, there is in osmic-acid preparations an appearance to be observed in the spindle-cells of the area adjoining that of the formation of vessels which is of special interest. In some of the larger spindle-cells, sometimes covered by the granular protoplasm of the cell, and sometimes lying partly in and partly outside it, are small rounded or quadrangular bodies, which in point of form, evenness, and colour, exactly resemble the red corpuscles in the bloodvessels at other parts of the field. Most of them are smaller than the average red corpuscles. When such a section is stained in red aniline, the blood-corpuscles appear of a characteristic yellowish red tint, and the same peculiar tint is to be observed in the bodies seen in the enlarged spindle-cells. Nothing else in the preparation is similarly stained.

I shall return to this appearance when I come to treat of the formation of new bloodvessels in inflamed tissue. Meanwhile, I remark that I do not claim that the formation of red blood-corpuscles in inflamed spindle-cells is proven, but I do maintain that it is highly probable, and invites to further and more searching investigation. There are two obvious sources of fallacy, against which I have been careful to guard. The spindle-cells of inflamed tissues sometimes undergo fatty degeneration and in osmic-acid preparations of such tissues the cells are more or less filled with small globules of fat—seen stained as black spherical bodies, much smaller than the bodies in question, more perfectly spherical, and much darker in colour. These, again, sometimes coalesce to form large fat globules, considerably larger than a red blood-corpuscle. The nuclear products of spindle-cells, which are identical in appearance with red blood-corpuscles, are not so uniformly circular, are not so darkly stained, and are not so numerous as the fat globules sometimes seen in the same cells.

Since I first observed this appearance, I have not had opportunity of subjecting such sections to the chemical tests, which may throw additional light on the matter.

Another source of fallacy is the possibility of red blood-corpuscles having entered the cells from without. I have on rare occasions observed in an inflamed cornea, in which there were no bloodvessels, red blood-corpuscles, which I thought could only have come from the conjunctival vessels. But I do not think this could have been the case with the bodies in question. They were not only at some distance from vessels, but they were many of them smaller than the average red blood-corpuscles seen in vessels.

After the bundles of the corneal tissue have been separated from each other by serous effusion, and the spindle-cells have become enlarged, and so altered that they are easily visible, a greater or smaller number of colourless blood-corpuscles enter the cornea from the conjunctival border. The length of time that the inflammation has lasted before this takes place to an extent that can be considered abnormal, varies according to the animal, and the degree and extent of the inflammation. The only positive data I have as to the question of time, were noted in esculent frogs in winter, the inflammation being produced by solid nitrate of silver, and limited to a small point in the centre of the cornea. I found after twenty-four hours that although the spindle-cells were visible, and swollen in sealed serum preparations, very few colourless blood-corpuscles (or wandering cells) could be detected, but after forty-eight hours, in many of the "spaces" nearest the corneal border one or two corpuscles could be seen. After four days their number was much increased, and from two to six could often be counted in one "space," and they could be seen near the centre of the cornea.

I employ the term colourless blood-corpuscle as it is perhaps the one in most general use, but the term lymph-corpuscle, which is also used, is probably a better one. "Leucocyte" and "white blood-corpuscle" are liable to the objection that the corpuscles are not white, and will both probably drop out of use. "Wandering cells," or shortly "wanderers," cells which Recklinghausen discovered in connective tissues, and which can be seen to possess the power of movement, have all the characters of colourless blood-corpuscles, and are, in fact, the same structures seen outside the bloodvessels in the lymph spaces of the tissues. Colourless blood-corpuscles, when they have lost their vitality, are seen as round granular bodies, and then constitute the structure which in pus receives the name of "pus cell." Set free on the surface of a mucous membrane either in a normal or pathological condition, the same colourless blood-corpuscle has been named a "mucous corpuscle." The sooner these various appellations are discarded in favour of a single one the better. They all refer to one and the same element, seen in various conditions.

To be able to tell what is and what is not a colourless blood-corpuscle in the inflamed cornea, is to be able to solve many difficulties, regarding which competent observers have formed very different opinions. Colourless corpuscles are liable to be mistaken for spindle-cells, and for nuclei of flat cells. I believe they can be distinguished by the following methods, which I have verified by treating portions of the frog's tongue, which I could see during the life of the animal to be crowded with colourless corpuscles which had escaped from the bloodvessels, the process of emigration having been observed under the microscope.

In the inflamed cornea colourless corpuscles can often be identified without difficulty when it is simply examined in aqueous humour, or better, examined after being one or two days sealed. But there are other appearances in such preparations, in regard to which it is very difficult to know whether they are colourless corpuscles or not. I believe that by this method it will be found difficult, if not impossible, to come to satisfactory conclusions regarding some of the changes which take place in the cellular elements.

In osmic acid we possess an agent which, in the present state of our knowledge, is indispensable in studying the colourless blood-corpuscles in the inflamed cornea. It fixes them in the form and size they possess in life. They appear mostly as round, finely granular bodies, the nuclei in which, without further staining, are not visible. When there are bloodvessels present, the colourless corpuscles present among the red corpuscles can be seen, and form a criterion by which the identity of those outside the vessels can be established. About their identity, there can indeed be no doubt, as the action of osmic acid on colourless blood-corpuscles is the same in all tissues. By all other methods, the diagnosis between colourless corpuscles and spindle-cells is difficult; by osmic acid their confusion is impossible. The spindle-cells, in addition to their shape, are seen to possess the characteristic terminal processes. The cornea may be for many days crowded with colourless corpuscles, and none of them show any transition to that form and condition. Subsequent staining, first with logwood and then with red aniline, shows well the nuclei of the colourless corpuscles. In such preparations, they are seen in the cornea to contain two to four rounded nuclear bodies, stained a colour in which both the dyes are combined. These nuclei are grouped in circles. The nuclei of the spindle-cells are grouped in rows. In a preparation so stained, and in which there are many colourless corpuscles and swollen spindle-cells, they can be identified each as belonging to one or the other class, even under a very low magnifying power.

The question, Whether, in the new growth that follows inflammation, spindle-cells are formed by colourless corpuscles, is not prejudged by these remarks.

In osmic-acid preparations, the flat cells are invisible.

When the eye has become familiar with the differences between colourless blood-corpuscles and spindle-cells in osmic-acid preparations, it becomes possible to distinguish them in gold preparations.

The colourless corpuscles in a tissue which has been treated by half-per-cent. solution of chloride of gold, and subsequent exposure to light in acidulated water, are seen as perfectly circular bodies, even and homogeneous, having only about two-thirds of the diameter of the same structures when seen in aqueous humour or osmic-acid preparations. They are surrounded by a very narrow darkly stained uneven border, which from its constancy is probably part of the cell-substance. The nuclei are usually invisible as individual elements. The effect of the gold has been to weld the nuclei and cell-substance into a compact homogeneous body.

Inflamed corneæ, treated by gold, and subsequent exposure to light in weak acetic acid, can be preserved indefinitely in alcohol, and sections from such corneæ stained in logwood show the colourless corpuscles as round blue bodies. In sections of an osmic-acid cornea, nothing is seen of the "spaces," but in a gold-stained cornea these can be seen, their position being indicated by the stellate nucleus. In a space there can frequently be observed lying beside the unmistakable large undivided nucleus of the stellate cell, one to three or four more deeply stained colourless corpuscles. These are most abundant near the border of the cornea. In the same preparations, the nuclei of the spindle-cells, generally divided, are also stained blue, being seen in groups of two or three, and recognised by the members of each group being placed end to end, and by the position of the groups being such that a straight line drawn through their longitudinal meridian would, if continued, cut them all into lateral halves. They are the visible divided nuclei of invisible chains of connected spindle-cells.

Inflamed rabbit corneæ stained in gold, and allowed to remain in weak acetic acid until their laminæ can be detached by the forceps, give preparations by which we learn how the colourless corpuscles enter the cornea. The laminæ, being sufficiently thin to be examined under the microscope, are stained in logwood, and the large nerve-trunks, if not otherwise visible, are then abundantly evident by the rows of stained nuclei. In a very successful preparation, the nuclei of the cellular sheath, which is continued on the nerve, are stained, as well as numerous nuclei on each medullated nerve-fibre. Between the cellular sheath of the nerve and the corneal substance by which it is surrounded, a perfectly colourless space can be made out, and in this space can be seen a varying number of stained colourless corpuscles. Sometimes they are seen in great numbers, and it can be observed that the "spaces" in the corneal tissue adjoining the nerve trunk contain them more plentifully than spaces which are more distant from it.

The same facts may easily be observed in the cornea of a rat. The whole eyeball should be placed in gold solution for an hour, allowed to remain twenty-four hours in acidulated water, the epithelium then removed from the corneal surface, and finally, the cornea excised, stained, and examined entire.

The nerve-trunks are accompanied up to the corneal border by bloodvessels, and white corpuscles which leave the bloodvessels at that part are immediately external to the nerve, and have a free passage into the cornea along its outer border. They also enter the cornea from the ordinary lymph spaces of the sclerotic and conjunctiva, the spaces of these tissues being continuous with those of the cornea.

The forms which the colourless corpuscles assume in the inflamed cornea, may be separated into two classes, one in which identity of the structure is still evident; and another, in which ulterior organic changes take place, by which the appearance is so changed that the identity of structure may well be challenged. The latter series of changes will be described in a future chapter, when I shall treat of the structural changes which take place in inflammation. The former I will now briefly enumerate, premising that the description applies solely to osmic-acid preparations.

In a section of a cornea which has been inflamed until there are new vessels fully developed, it can be seen that the colourless corpuscles in the tissue are somewhat larger than those in the bloodvessels; and that of those free in the tissue some are larger than others. A great majority of the corpuscles are round; but sometimes, when they are in a layer, they have a hexagonal form, produced by mutual pressure. Others, lying singly, have forms more or less irregular, being club-shaped, or having rounded projections. Sometimes, on either side of a bloodvessel filled with red blood-corpuscles, they form a continuous column, as if they were contained in some tubular structure. They are more rarely seen in straight rows. When they are seen in this position, they touch each other, and are in numbers far too great to suggest that they can account for the spindle-shaped figures frequently seen in gold preparations; and which we have besides learnt to recognise in osmic-acid preparations as spindle-cells. The straight rows probably correspond to the longitudinal spaces between the primary bundles. They are thus seen in three different positions relative to the corneal structure—namely, as groups confined to the "spaces," in layers, and in rows. The first occurs earliest, the layers and rows being seen only after the inflammation is acute, and has lasted for some time.

Sometimes a colourless corpuscle can be seen as a double body in osmic-acid preparations. Two rounded extremities, granular, and each about the size of a single corpuscle, are joined by a narrow band or isthmus. This connecting isthmus is not granular like the rounded extremities, being even and homogeneous in appearance. In logwood and aniline stained preparations, nuclei are seen in either extremity, but not in the isthmus.

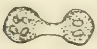

Fig. 31.—Colourless blood-corpuscle probably dividing into two. From inflamed cornea of rabbit. Osmic acid. Logwood. Red aniline. Mag. 450 diam.

In judging of the signification of these double corpuscles, it is necessary to bear in mind observations that have been made by Stricker, Klein, and Ranvier. In the inflamed tongue of the frog Stricker[1] saw the division of a colourless corpuscle into two in the living tissue. Klein[2] observed a similar division in the blood of the newt examined on the warm stage. Ranvier[3] observed the division in the blood of the axolotl, examined in the moist chamber, and gives a series of figures showing the stages of the division.

It is highly probable that the double corpuscle, seen in the osmic-acid sections, has been surprised by the reagent in the act of preparing to divide, and been "fixed" in the attitude.

As each of the rounded extremities of this double corpuscle is equal in size to the majority of the single corpuscles, it follows that the division is preceded by growth of the original cell.

In some osmic-acid sections, colourless blood-corpuscles are to be seen, which contain several nuclear bodies, which, judged by the tests formerly applied to similar appearances seen in some spindle-cells, are probably of the nature of red corpuscles. I have satisfied myself that these are not instances in which the red corpuscles have been taken into the substance of the colourless corpuscles from without, nor are they products of fatty degeneration of the cell. The relation which this fact has to the difficult problem of the genesis of the red blood-corpuscles is too important to be lightly passed over, but want of space prevents my entering on the subject more largely here. It may be, however, of interest in this connexion to call attention to an observation by Eichhorst.[4] He found, during five days, in the blood of a patient in the second week of typhoid fever, finely granular colourless cells, the diameter of which was four to six times that of colourless blood-corpuscles. These cells contained two to five, rarely seven, discs, which perfectly resembled the red blood-corpuscles, except that they were somewhat paler. By pressure, or the addition of water, they were set free from the mother cell.

Laptschinsky,[5] in studying the microscopic appearances of the blood in fevers, observed colourless corpuscles which contained nuclei deeply tinged with the colouring matter of the blood.

So much for what I believe can be ascertained regarding colourless blood-corpuscles in the inflamed cornea. If gold preparations are controlled by osmic-acid preparations, it will be seen that spindle-cells have often been confounded with them. For example, in Plate XVI., Fig. 7, illustrating Axel Key and Wallis's[1] paper on Keratitis, it is not difficult, after an acquaintance with osmic-acid preparations, to recognise in the thickly-set elongated cells, spindle-cells in which the connecting processes are invisible. But the authors' description is, that these are "spindle-shaped in-wandered cells arranged at right angles to each other." The drawing is an accurate illustration of appearances that are frequently seen in gold preparations, but more perfect methods show that the interpretation given to it is incorrect.

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[1] Studien, etc., loc. cit.

[2] Centralblatt, 1870, p. 17.

[3] Archives de Physiologie, No. I., 1875.

[4] Deutsches Arch. f. Klin. Med., 1874, XIV. 223.

[5] Centralblatt, No. 42, 1874.

In the next chapter, I shall endeavour to collect into short compass what is known regarding the passage of the colourless corpuscles from the interior of the bloodvessels into the interstices of the neighbouring tissues, and sketch the history of the discovery of this important accompaniment of inflammation.

ARTICLE IV.—*On the Pathology and Operative Treatment of "Hip" Disease.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

(*Continued from page* 598.)

THE results of my twenty-two cases of excision of the head of the femur are as follows:—

Fourteen recovered completely, with a useful limb and movable joint.

Three recovered from the operation, but died within eighteen months from visceral disease.

Five died; one fifteen months, one twelve months, one eleven months, one five months, and one three months after the operation, from the combined results of the operation and visceral complications.

It will be observed that no case died from the *immediate* effects of the operation.

These results contrast most favourably with those published by other surgeons; and there are two causes which I consider have contributed to my success in this operation. These causes are—1*st*, The employment of antiseptic treatment; 2*d*, The performance of the operation at an early stage of the disease.

The employment of the antiseptic precautions, originated and practised by my esteemed colleague Mr Lister, is now so much a matter of course in my surgical practice, that no special reference is made to it in the brief notes of the cases reported.

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[1] Virchow's Archiv, Band 55.

From a study of my more extended notes of these cases, I find that thirteen of the twenty-two were treated antiseptically, and that *all* of these made a complete recovery. The nine remaining cases were not treated antiseptically, four of them being treated before antiseptics were used in my wards, and the other five, being complicated with open sinuses or bed-sores, could not be satisfactorily treated in this way.

Accordingly, thirteen cases were treated antiseptically, and nine non-antiseptically. All of the former recovered ; all of the latter, except one, are included in the unsucessful cases, classed under the second and third series.

These facts are important in connexion with antiseptic surgery, and also in connexion with the early performance of the operation ; for in the non-antiseptic and unsuccessful cases, the disease was, in the great majority of them, more advanced than in the successful ones.

In regard to the second cause, I have to remark that, in seventeen of the cases, no external sinus existed, although there were signs of, and suppuration in all. Out of these seventeen, four were unsuccessful ; but it is most interesting to note that these four were among those treated non-antiseptically. In the remaining five, external sinuses existed, and all of these were unsuccessful, except one.

What cases, then, of "hip" disease are suitable for operative interference? Hitherto, surgeons have usually hesitated to perform excision in cases of hip disease until the disease was in an advanced stage, and external sinuses existed. The chief reason for this delay has, in my opinion, been the uncertainty as to the real condition of the joint, and also some uncertainty in regard to the exact pathology of hip disease.

The general practice has been to open abscesses, keep the joint at rest, attend to the health, and wait until the disease has further developed itself. Then, and not till then, has the question of excision been considered. No doubt, some cases treated in this way recover with limbs more or less useful, and joints more or less stiff; but most surgeons are agreed as to the fatal nature of cases of suppurating hip disease, and also as to the results of such favourable cases being less satisfactory in regard to the mobility and usefulness of the limb than those following successful excision.

Now that we have in an antiseptic exploratory incision a certain and safe means of diagnosing the exact condition of a diseased hip or other joint, surgeons have no longer an excuse for not acquiring a sure knowledge of the disease ; and if my observations as to affections of the head of the femur being the most common origin of hip disease be correct, the natural practice certainly is to remove without delay the source of irritation, when it has been examined, and its state correctly discovered.

I advocate, then, an antiseptic incision in all cases of hip disease in which signs of suppuration are present; and if an examination of the joint by this means determines that destruction of the articular cartilage of the head of the femur and caries of the bone exist, I advise the immediate excision of this bone, believing that the removal of this bone will in the large majority of cases check further disease, and allow the patient to recover with a useful and movable limb. Should the disease be limited to the head of the femur, and not involve, or only affect slightly, the acetabulum, it is, in my opinion, only necessary to take away the head of the bone by sawing through its neck. If more than the head of the femur is affected, or if the acetabulum is deeply involved, then it is advisable to take away the great trochanter as well, in order to allow the free escape of pus from the acetabulum, or from the pelvis, if the former should have become perforated.

Cases of acute and rapid disease of the hip-joint have generally been considered unsuitable for the operation of excision, but the complete success obtained in case 18, which was a most acute form of the disease, proves that this idea has been erroneous.

When the antiseptic examination of a suppurating hip-joint determines that the articular cartilage and bone are unaffected, or only slightly involved, the excision of the bone will not be required; for a careful antiseptic treatment of the wound, with perfect rest of the joint, will, I believe, result in a good recovery in most cases. If, however, the articular surfaces are markedly involved, I strongly advocate excision, as I do not believe that even the use of antiseptic treatment will cure disease of the cartilage or bone, when it has once become thoroughly established, so as to leave a movable joint.

The excision of the head of the femur in more advanced cases of hip disease, and where external sinuses exist, although not so favourable in its results as when performed in the earlier stages of the disease, is a proper and justifiable operation; and I believe that even in these cases much may be done to lessen the mortality by thoroughly scraping the lining membrane of the sinuses according to Volkmann's plan, and then dressing the wound antiseptically. In such cases it is important to take away the diseased bone freely, so as to remove as completely as possible all source of irritation, and also to allow the free escape of pus from the acetabulum or pelvis.

Before performing excision in any case of hip disease, it is essential to ascertain the condition of the internal viscera. Should these be seriously diseased, operative interference is not advisable; but I do not myself consider slight and inactive affections of these viscera as necessarily a bar to operative interference, especially if the local disease is causing profuse discharge or irritation.

Method of Operating and After-Treatment.—Abscesses connected with hip disease require to be opened in the most suitable situation, and external sinuses enlarged if need be; but when making an exploratory incision into a suppurating hip-joint, I find it most convenient to enter the knife immediately above, and in a line with, the posterior margin of the great trochanter, and to make an incision long enough and deep enough to allow the finger to pass into the joint. If, then, it is considered advisable to excise the head of the femur, sufficient space will be obtained by extending the wound upwards and downwards for about two inches each way. Should the trochanter require to be removed, the incision should be carried downwards an inch or two more. If any portion of the acetabulum requires to be taken away, an incision across the centre of this longitudinal one will best expose the cavity. Having in the incision advised divided the skin and cellular tissue, I introduce a strong probe-pointed bistoury, and cut across the attachments of the muscles close to the bone, saving their periosteal connexion as much as possible. This division of the muscles allows the head of the bone to be turned out to a sufficient extent, and its neck sawn through by means of a fine narrow saw. In this way the head of the bone can be removed with little disturbance of the surrounding parts,—a circumstance which must assist in making the resulting new joint more perfect.

If the trochanter and neck require also to be sawn off, the division of the muscular attachments must be more free; but, even in this case, I think it of consequence to interfere as little as possible with the neighbouring tissues. Few or no arteries of any size are wounded in this method of operating, and it frequently occurs that not even one ligature is required.

In my early cases I found that the posterior flap of the wound had a great tendency to fall down, and so separate its edges; but I now find that the introduction of two or more button-sutures in addition to the ordinary superficial ones successfully counteracts this.

I have further to add, that I strongly advocate antiseptic precautions during the operation, and the antiseptic treatment of the wound afterwards, and also note the importance of having a free escape—by the use of drainage-tubes—of pus or other fluids from the deep parts of its cavity. In addition to the usual antiseptic treatment, I always apply freely to the exposed articular surfaces or bone De Morgan's solution of chloride of zinc.

The after-treatment of the operation is of the most simple kind, and consists in supporting the limb in the extended position with one or more sand-bags or pillows; or, if there is any tendency to contraction, drawing up of the limb, or painful startings, the application of extension by means of a light weight is useful. Gentle movements of the new joint should be commenced at the end of three weeks, unless there is any painful symptom or condition of the wound which forbids it; then the movements should be employed as soon as this condition or pain has passed off. When the wound is healed, or nearly so, the patient may be allowed to sit up and use crutches. In cases in which the limb was weak, I have used with advantage some form of wire or other splint while the patient was in the upright position, but in the majority of my cases this support was not required.

The time of recovery after excision of the head of the femur varies according to the condition of the parts before the operation and strength of the patient. The progress is usually slow, and I should say that from four to five months is the average time that a case of hip-excision requires for its recovery. Should the wound be slow to heal, or should sinuses continue to discharge, a careful search with the probe or finger ought to be made; for portions of diseased or dead bone are frequently the cause of this condition, and their removal is necessary for the proper healing of the wound or sinus. When the patient is able to bear weight on the limb, it is necessary to add to the boot or shoe a raised sole of light material, in order to compensate for the shortening resulting from the operation. This shortening is usually from two to three inches.

My opinion on the pathology and operative treatment of hip disease may be briefly stated in the following resolutions:—

(1.) That although hip disease may commence in the synovial membrane or pelvic bone, it originates most frequently in the head of the femur.

(2.) That the early, sure, and safe detection of the exact condition of a diseased hip or other joint, when symptoms of suppuration are present or are doubtful, by means of an antiseptic exploratory incision, is a valuable improvement and aid in the treatment of joint affections.

(3.) That the early excision of the head of the femur, when diseased, is the treatment most likely to check the progress of hip disease, and to cause the patient's recovery, with a movable and useful limb.

(4.) That, therefore, when signs of suppuration exist in connexion with hip disease, an exploratory incision should be made, and the condition of the joint determined.

(5.) That if the articular surfaces are found to be unaffected, means should be used to favour the free escape of pus or other fluid from the joint, and care taken to keep the articulation perfectly at rest.

(6.) That if the articular cartilage of the head of the femur is found to be destroyed, and the disease is limited to this portion of bone, excision should be at once performed by sawing through its neck.

(7.) That if the disease affects more than the head of the femur, this portion of bone, together with the neck and great trochanter, should be excised; and any diseased portion of the acetabulum also taken away.

(8.) That in all operations on the hip or other joints, antiseptic precautions should be carefully employed during the operation, and until the wound is healed, as this treatment is the most certain known means of lessening the mortality.

ARTICLE V.—*Closure of the Genital Fissure as a means of Treatment in Cases of Vesico-Vaginal Fistula with extensive Destruction of the Substance of the Bladder and of the Anterior Vaginal Wall.*

By JOSEPH BELL, F.R.S. Ed., F.R.C.S. Ed., Surgeon to the Royal Infirmary, and Lecturer on Surgery, Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh, 1st December* 1875.)

Note on Previous History.—Operations of this character seem to have been of rare occurrence. Vidal (de Casis) performed it for the first time at the "Midi" in 1832,—Wutzer having in the same year attempted it at Bonn, but unsuccessfully.

Berard, in 1844, performed it in a very bad case, which did well for three weeks, but died thirty-eight days after the operation, of an acute peritonitis, though the operation seemed to have succeeded. He reported his case to the Academy of Medicine, and in the discussion which followed,[1] the operation was universally condemned by all, as having killed the patient, as a certain failure, as neither rational nor "*convenable.*"

Few cases are found in more recent literature till those of Bozeman[2] in 1867; Simon,[3] 1868; and latterly those of Herrgott of Nancy.[4]

Objections.—There are many objections which may be raised to this operation in any of its forms. 1. *Danger:* Several of the cases have died—two at least of peritonitis. 2. Subsequent sterility and incapacity for copulation. This is hardly a valid objection, as no one would propose to operate except in the most extreme cases, in which the patients are practically sterile from the disgusting nature and great extent of their deformity; and in all cases, the surgeon is bound most fully to explain both to the patient, and, if married, to her husband, the consequences of the operation.

In the minor cases in which merely the upper portion of the vagina is interfered with, though the patient is left sterile, and menstruation takes place through the bladder and urethra, there still remains a few inches of vagina, and the comfort obtained is very great.

Herrgott of Nancy[5] reports a case of this nature which seems to have resembled closely the first one I operated on. His, in abstract, is a case of vesico-utero-vaginal fistula, three inches long;

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[1] Gazette Médicale de Paris, 1845, p. 126.

[2] N. Y. Record, 1867.

[3] Deutsche Klinik, 1868, Nos. 44 and 45.

[4] Archives de Tocologie, Juin 1875.

[5] Op. cit.

neck of uterus deformed by cicatrices; bladder formed a hernia into the vagina. Three operations were performed: by the first, the right portion was united; by the second, the left; and by the third, a small central aperture was closed: menstruation took place by the bladder.

CASE I.—Mrs ——, æt. 35, mother of three children, received injuries in her last labour, which caused great destruction between the bladder and vagina. Before I saw her in February 1869, she had been operated on three times by Dr M. Duncan, and twice by a surgeon in Glasgow. Much of the fistula had been united, but there still remained a deep and broad aperture, about three-quarters of an inch in length, close up to the anterior lip of the os uteri, in the very summit of the anterior cul-de-sac. The anterior lip was atrophied, and completely denuded of mucous membrane, so the only chance of stopping up the gap seemed to be to pare the edge of the vaginal wall in front, and also that of the *posterior* lip of the os uteri, and unite them; by this means, putting the os uteri into the bladder, causing an artificial retroversion of the uterus, and trusting to the patient's menstruating through her bladder.

24th February.—Operation was difficult and tedious, from the fact that a speculum could not be used, as it forced up the uterus; so pulling down the posterior lip by a pair of tonsil forceps, I managed to pare it freely; the other edge was more easily managed; and then I passed four silver wires deeply through both edges by my needle-points. The operation lasted one and a half hours, but the aperture was completely closed, and a catheter was inserted. For six days the patient was quite dry, but on 1st March, some urine came through vagina. On removing the stitches on the ninth day, the wound was found to be completely closed, except one small point in the centre. Patient was sent home, to return in a few weeks.

8th June.—Patient returned. The uterus is found retroverted. The os is almost concealed in the bladder. Posterior lip alone projects through a small central wound. She can retain her urine perfectly when in bed.

12th.—I again operated, laying very freely bare both surfaces of the fissure to the extent of fully an inch in its transverse diameter, and again secured it with four deeply placed strong sutures.

22d.—She has been quite dry since the operation, and stitches have been safely removed.

The result was a complete cure of the incontinence of urine, with an excellent, somewhat shortened, vagina; and menstruation occurred through the bladder, with some little difficulty when clots passed.

In Professor Herrgott's case, menstruation occurred soon after one of the operations, occasioning considerable anxiety and much pain. Subsequent menstruations after the case was complete were less painful, and really gave very little inconvenience.

A second and less severe form of the operation is possible in cases where the bladder is extensively destroyed, but the urethra still remains healthy and uninjured, and with its sphincter power unimpaired.

In such cases it may be possible to close the vagina in such a manner as to utilize its posterior wall for bladder purposes, and yet to preserve a small cul-de-sac outside the utilized portion as a vagina, while the patient is able to retain urine, and pass it through the urethra.

Probably those cases in which sloughing has taken place to a great extent, and its cicatrization has caused a great narrowing of the vagina before the surgeon sees the case, will most frequently and most successfully be treated in this manner.

Of this variety seems to be Case XI. in Herrgott's paper. In it, after great loss of substance, the vagina was found to be closed by a cicatricial membrane, with the exception of an opening which admitted two fingers; this was eventually entirely closed by three successive operations.

But the class of cases of which I now wish to speak, two of which have come under my own care, are of a much more serious and distressing nature.

In them, not only has the bladder wall been destroyed, but also the urethra has shared the same fate. In them we have a hernia or prolapsus, or even rather an inversion of the remains of the bladder wall against the posterior wall of the vagina. Great misery results from the constant dribbling, the pain of the bladder in its abnormal position, and the abrasion and ulceration of the thighs.

There seems no possible operation which can restore to these victims the sphincter power which they have lost. What we are to aim at is to enable them to wear some apparatus by which the urine may be collected, and their life rendered less wretched.

In both the following cases, the condition was aggravated, and the hernia of the bladder increased in amount, by the fact that the perineum had been lacerated at the time of delivery.

In both, the resources of surgery had been exhausted before I saw them, and both were willing to submit to anything that would promise any alleviation of their sufferings.

Both were unmarried women, and, without the slightest hesitation, accepted the proposition that their genital fissure should be as far as possible closed.

My idea was, to split up both labia in such a manner as, without removing any of the mucous membrane, I should leave a broad vascular surface, which should extend from the posterior fourchette forwards on both sides almost to the mons veneris in front.

That these prepared surfaces should be brought together with extreme care by three sets of sutures:—

1. The mucous membrane or inside edge of each to be approximated by closely-set catgut sutures, which might be trusted to absorb in time.
2. The broad surface exposed of each labium to be brought together by deeply-placed and close silver-wire sutures, secured over quills or buttons, which may be removed on the seventh or eighth day.
3. The exact edge of the skin to be approximated by horse-hair or silk sutures, to be removed whenever union was secure, or irritation caused.

That menstruation and micturition were to be provided for by leaving a narrow opening at the upper angle of the genital fissure, to be kept open during the healing process, by a large well-balanced lead catheter of a double sigmoid curve.

The patient afterwards to wear such a catheter in silver, fitted on to a urinal in the form of a boat-shaped shield.

Case II.—M. W., æt. 22, admitted 3d September 1874, dismissed 2d February 1875.—To-day, 14th September, patient being put under chloroform, Mr Bell split up the edges of the labia and brought them together with numerous catgut silver and hair sutures; thus closing up the whole of the genital passage, with the exception of a small opening, large enough to admit a male catheter, for the escape of the urine. Patient had to get a little opium, on recovering consciousness, on account of the pain continuing. Is doing satisfactorily since.

15*th.*—On evening after operation had slight rigor. Chloroform is administered to patient about twice a week that the parts may be washed with a weak solution of permanganate of potash; after the catheter has been replaced, the patient remains dry for about twenty-four hours, though after this a little urine begins to ooze. Deep sutures taken out, Friday evening 26th, when things, on the whole, looked well. The parts are lubricated with zinc ointment to prevent excoriation by the urine. The union became perfect, except that a small aperture remained near the edge of anus.

24*th November.*—Patient operated on again to-day. Edges of aperture pared and brought together with silver stitches. No bleeding. A catheter was introduced and left.

17*th January.*—Patient going on well.

30*th January.*—Patient has frequent severe pains (neuralgic) in the hypogastrium shooting down the thighs, probably connected with ovarian affection. She says she is always this way when she is unwell.

1*st February.*—Patient up to-day. Finds Mr Gardner's apparatus of much service in keeping her dry, as long as she is in an erect posture.

2*d February.*—Patient went away to the country that afternoon, in great comparative comfort.

The same history may substantially suffice for another case exactly similar. M. A., admitted 26th December 1873; dismissed in great comfort, 19th February 1874.

While on this subject, I may briefly note that a fortnight ago, I saw a patient in whom, nearly three years ago, I had closed a large vesico-vaginal fistula in the ordinary way with success. After some time, however, she has again found herself unable to retain urine. I found an aperture as large as a small pin's head, evidently a needle-hole, through which urine escaped in a fine stream. I have closed this by one application of the actual cautery.

ARTICLE VI.—*The Treatment of Nævus.* By JOHN DUNCAN, M.A., M.D., F.R.C.S.E., Assistant-Surgeon Royal Infirmary.

SINCE, ten years ago, I pointed out the value of electrolysis in nævus and allied disorders, I have had considerable experience in the treatment of this class of disease, and have necessarily been led to certain conclusions. It may, perhaps, be useful to formulate them, because the methods of treatment which have been proposed are exceedingly numerous, and each has been advocated more or less strenuously as universally applicable and absolutely the best; so that the practitioner who has a case before him is embarrassed by the very plethora of measures from which he has to choose.

It is important that the natural history of nævus be distinctly borne in mind. It is a disease of uncertain habit, prone to fits and starts, and it is impossible to say of any one example that it will progress, remain stationary, or wither away. There are nevertheless certain probabilities in this respect which deserve remark.

The disease may or may not be congenital. Sometimes at birth there is an indication of its presence; as often there is not. Within a few weeks after birth, however, the character and extent of the deformity have usually fully declared themselves. Then there is a pause. I have not myself observed retrogression at this stage, but almost invariably the nævus either ceases to enlarge, or does so for a time very slowly.

The period of the first dentition is the next important epoch in the history of nævus. A large number then retrograde. My own experience is that about one-third do so. Some entirely disappear. Some shrivel and diminish. Some abandon the subcutaneous tissue, but leave a stain upon the skin. Some degenerate in a fibrous or calcareous manner, or undergo a cystic transformation. Of the remaining two-thirds, fewer and fewer continue to increase as time goes on; now and again one dwindles away. At the second period of dentition, and again about puberty, a further marked diminution takes place; and at length in adult life, partly, no doubt, from the efforts of the surgeon, but greatly also through the vis medicatrix naturæ, it is rare to find any other form of nævus than the port-wine staining of the skin—a form of which it may be affirmed that while, on the one hand, it is little prone to those fits of growth which characterize the subcutaneous tumour, it is, on the other hand, much less likely to disappear.

Such being the natural history of the disease, it is an important practical point to determine the dangers and inconveniences which attend it, if it be left to run its course. They may be included under three heads—deformity, ulceration, and hæmorrhage. Of these, deformity is the cause for which the surgeon is most frequently requested to interfere. Ulceration of the surface may be produced by external irritation, sometimes it arises without obvious cause, but it rarely becomes troublesome. In most instances the ulcer heals on the removal of the cause and appropriate medication. Not uncommonly it exercises, by inflammatory action or by the contraction of its scar, a beneficial influence on the growth, and I have seen even complete cure brought about by the friction of a flannel binder.

Dangerous hæmorrhage rarely results either from ulceration or injury. I have on several occasions seen severe wounds of nævi, and although they bled pretty freely at the time, the bleeding was easily controlled by pressure. In one instance, repeated hæmorrhage of small amount took place from an ulcerated surface, and the patient became much exhausted. That, however, is the only case I have met with in which there was the remotest suggestion of a fatal result.

One other source of danger there may be in the tendency which some nævi have to pass into the allied condition of cirsoid aneurism. But even that transformation is not of serious import; for I firmly believe that many of the severe measures which have been adopted for cirsoid were not justified by the position of the patient.

Taking all these facts into consideration, it is impossible to avoid coming to certain conclusions which would tend greatly to limit the sphere of surgical interference. It may be safely asserted, in the first place, that it is impolitic to interfere, unless the nævus be extending its area; and, in the second place, that no method of treatment is justifiable which distinctly involves danger to life.

For practical purposes, nævi may be divided into two classes—those which are stationary, and those which are spreading.

Of the former, so large a number disappear, and the cure by nature is so much safer and better than the cure by art, that the first proposition which I have formulated admits of few exceptions. Two exceptions may be made, viz., (1) when the nævus is exceedingly small; and (2) when the means of cure are absolutely innocuous.

Nævi about the size of a pin's head, and seated on the skin, are not uncommon on the face. They sometimes spread, they are very enduring, and it is therefore well to destroy them while yet small. The best method is to puncture the little spot with a needle previously dipped in strong nitric acid. This is simple, certain, and leaves no visible mark. It may be destroyed with equal certainty by the heated needle, by the galvanic cautery, or by electrolysis; but these measures are inferior in simplicity, and are prone to leave a more distinct cicatrix.

Of all the modes of treatment which have been proposed, pressure is probably the only one which can be called absolutely innocuous. Unfortunately, it seems equally innocuous to the nævus and the patient. Even its advocates demand a suspiciously long time to effect a cure thereby. It is evidently only in some situations that pressure can be continuously kept up; and in these the prolonged annoyance from the use of the necessary apparatus outweighs the slight chance of cure. I have sometimes, when a nævus of small size is situated over a bone, as on the forehead, advised the application of collodion every second or third day. A considerable number gradually disappear during the process; but I have not satisfied myself that the disappearance is its effect. It has, perhaps, the merit of keeping the patient's friends for a time harmlessly amused.

From this verdict of non-interference in stationary nævi, yet another exception must of course be made. I allude to those which persist beyond the age of puberty. The hope of a natural cure is then gone, and the deformity which they produce thus becomes a justification for treatment. They may be referred to the same category and be subjected to the same remedies as the spreading nævi which we have now to consider.

In this, the second class into which I have divided nævi, it is perfectly justifiable to interfere. Extreme slowness, or extreme rapidity of increase, may sometimes warrant delay. Thus, if the growth be very slow, and getting slower, it may be right to await a possible retrogression for a limited time. In other cases, from the very rapidity of growth, it is difficult to see how any interference can be effectual. I have watched a case in which, at birth, the only trace of nævus was a small mark on the temple, supposed to have been produced by a bruise in delivery. Within a fortnight, little patches of nævoid skin were observed over the side of the forehead, head, cheek, and palate, which speedily coalesced and formed a prominent mass, closing the eye, pushing forward the ear, and greatly enlarging the upper lip. It was impossible to contend with the growth; but it is satisfactory to be able to add, that now, three years afterwards, it has spontaneously almost disappeared.

With these exceptions, however, I believe that we ought to attack every spreading nævus. To this variety are due what risks in the way of hæmorrhage and ulceration may attend the disease; and while, no doubt, the majority would, if left alone, end by disappearing, yet, in the remainder, the operation which has ultimately to be performed becomes more difficult and dangerous exactly in proportion to the size of the growth. At the same time it must, as already said, be borne in mind in choosing measures, that our justification in operating is the deformity and inconvenience, rather than the danger, of the disease; and that, although these may warrant us in running a moderate risk, it is a strong argument against seriously hazarding the life of the patient. The same considerations ought also to make us careful not to undertake an operation, of which the result is more unsightly than the disease; nor, in eradicating swelling and discoloration, to leave a face slashed with scars, "as if it had been operated upon for double harelip."

The means at our disposal may be divided into two classes—(1) those by which a scar is avoided; and (2) those which necessarily leave a cicatrix of greater or less magnitude. If these were of equal value in other respects, it is plain that the former ought always to be preferred. But, unfortunately, this is not the case. Those which involve destruction or division of the skin have the advantage of greater certainty and rapidity in producing a cure; and although the subcutaneous methods of treatment are in most cases less dangerous, this quality is not sufficiently pronounced to render them preferable when a scar may be unimportant. I think a general rule may be laid down, that in operations on the face and other exposed portions of the body, the first class ought to be used, because they avoid disfigurement, and because, although a single operation may not suffice, repetitions in the end succeed; but that, on the other hand, the more rapid and certain methods should be applied elsewhere.

The first class aim at the destruction of the texture of the nævus, or the coagulation of the blood which it contains, by agents which can be introduced through minute punctures of the skin. The means used are the injection of coagulating fluids, the introduction of setons, the subcutaneous ligature, electrolysis, and the galvanic cautery.

Many different fluids have been used as coagulants. They all have a double action, in securing coagulation, and in setting up an inflammatory action, which tends to obliteration of those vessels which may have escaped their direct effect. The most reliable appear to be perchloride of iron and carbolic acid. Their special advantages are that they are powerful coagulants, and that while their caustic properties excite inflammation, this inflammation has little tendency to pass beyond the adhesive type. One danger attends them which limits their applicability. A considerable number of cases have been recorded in which a rapidly fatal issue has followed the injection. This appears to have been due to the escape of the fluids or clots beyond the limits of the nævus, and consequent coagulation of the blood in parts essential to life. At all events, it is certain that, unless the circulation through the nævus can be thoroughly controlled by temporary ligature or otherwise, injection ought not to be performed. Another danger, or rather difficulty, also deserves attention. It is by no means easy so to apportion the amount of injected material to the size and viability of the tumour that, on the one hand, the action may be sufficient to obliterate the vessels; and, on the other, not so severe as to cause the whole mass to slough off. I have seen in careful hands, in one instance, the upper lip destroyed; and, in another, ectropium produced by destruction of skin in the eyelid. Care should be taken that the fluid injected be distributed over the growth in minute portions; and the surgeon should be careful to do less at one sitting than is necessary for obliteration, trusting rather to repetitions of the operation, which ought not to be made at too short an interval. I believe that a preliminary disruption of the texture of the nævus with a tenotomy knife is advantageous, by permitting greater diffusion of the coagulating fluid, and therefore a greater effect with a smaller quantity than otherwise would be the case. With these precautions, that the circulation be controlled, and the amount injected kept within due proportions, I believe this method of treatment to be the best we have for nævi of moderate size situated on the face, if they be mainly subcutaneous. It is safe, very successful, and leaves no scar.

The passage of setons, and the subcutaneous ligature, are remedies allied to each other in their effects and mode of action. They obliterate vessels, excite inflammation, and bring to bear on the growth the contractile effect of a scar. Setons, whether simple or steeped in an irritating and coagulating fluid, appear to offer no advantages over, and in some respects are decidedly inferior to, the subcutaneous ligature. They are inferior in the large number of suppurating orifices, and consequent cicatricial marks which they produce. The subcutaneous ligature, moreover, has the advantage of at the same time exciting inflammatory action, and cutting off a large part of the vascular supply. It may be made of various materials—cord, silk, or wire. I have lately used the elastic ligature with good effect. To establish it in position, a strong cord must first be passed round the growth subcutaneously. This can be easily done in the largest nævus by bringing out the needle once, or at most twice, in the course of its passage round the circumference. But if it be intended to draw through an elastic ligature by means of the cord, the needle in a large nævus should be brought out three, or even four times, that too great a strain may not be made on the elastic. Lest even then the elastic ligature should break during traction, I have always left the cord long enough to be attached to another. When the ligature has been established, the cord may of course be withdrawn. The advantages of the elastic material are the continuous tension which it maintains, and the ease with which the tension can at any time be increased. In order to prevent the knot being drawn under the skin, I use a metallic button with slits, into which the ligature passes when stretched, and which accordingly holds it when relaxed. To render it still more secure, the ends may be tied together over the button.

The method of subcutaneous ligature is not without its drawbacks. It necessarily produces a certain amount of suppuration, and in some cases this is accompanied by considerable constitutional irritation. I had lately a large nævus of the back sent to me by Dr Macnee of Inverness. It measured 4 by 3½ inches, and was of great thickness. Dr Macnee had already used setons, but although far a time they appeared to do good, ultimately the nævus increased again with great rapidity. I established an elastic ligature which came away in ten days. During that time the child (a few months old) was restless, feverish, and had a good deal of diarrhœa, as had also been the case when setons were used. The nævus shrivelled and diminished greatly, but I have since heard that it has not disappeared. I mention this case as the most unfavourable I have had, because it shows that the result of the subcutaneous ligature is not absolutely certain, and that sometimes it causes anxiety from constitutional irritation—an effect which we might expect from the analogy of teething. I am of opinion, however, that these large nævi are best treated in this way. Excision would be a severe operation in so young a child, and the raw surface left by external ligature would be very great. On the other hand, had the case I have mentioned, in which I was warned of the constitutional irritability, come before me now, I should have proceeded by the next method to which I have to refer — electrolysis, as being, although tedious, not prone to cause constitutional disturbances.

Electrolysis may be performed in two ways, viz., with and without insulation of the electrodes. When the needles are not insulated, the operation consists simply in causing more or less complete sloughing of the nævus. The sloughs are precisely similar to those made by caustic acids, if the positive pole be used mainly or alone; to those made by caustic alkalies, if, as is better, the use of the negative predominate. This method, therefore, is one that leaves a scar, and offers no advantage over destruction of the growth by caustic, except, perhaps, in being somewhat less painful; no advantage over ablation, except that the scar is somewhat smaller. I have used the method, when the skin was largely involved, by passing fine needles under and parallel to the skin. It occasionally does good, especially when combined with other measures; but really no treatment, except entire destruction of the skin, is thoroughly reliable in these cutaneous stains. Electrolysis with uninsulated needles has a very limited application. Electrolysis by means of insulated needles might be regarded as our best method of curing nævi, but that it is more tedious than injection, less certain than ligature. The needle is insulated by means of vulcanite. The exposed point is made to correspond to the size of the nævus, and care is taken to bury it completely in the growth. The strength of current may vary between three and twelve cells of Weiss's Smee's battery, according to the distance between the poles, and whether or not anæsthetic be used; and the duration of the action is determined by the feeling of hardness produced. Both poles should be inserted, because the current is then more effective, *i.e.*, the resistance is diminished, and there are two points of action. The number of needles will vary with the size of the tumour. I have already, in a former paper, related a number of successful cases. I would now merely summarize the conclusions of a fuller experience.

1. In a large number of operations, I have never met with an untoward result. In two cases of nævi situated on the head, I have observed alarming pallor come on suddenly during the operation, but I have been inclined to attribute this to an overdose of the anæsthetic, because in the same patients subsequent operations were performed without this disagreeable concomitant. There is a tendency to administer an overdose, for the patient, when apparently fully anæsthetized, is often roused by the closure of the circle, and more chloroform is given to quiet him. I may remark, that it is generally necessary to anæsthetize. The operations are prolonged and painful.

2. With moderate care, sloughing can be avoided, and suppuration ought never to be seen.

3. With perseverance a cure may always be effected.

4. But, on the other hand, electrolysis by this method is extremely tedious and troublesome. I find from experience that no advantage is gained by repeating the operation oftener than once in three or four weeks, and that in a nævus of moderate size five or six operations are required.

5. I have already referred to the difficulty of curing a nævoid discoloration of the skin. The remark applies to the treatment by electrolysis, as well as to all other methods. A case sent me by Dr Williamson, of Leith, illustrates this. The entire parotid region, auricle, and upper part of the neck on the left side, were occupied by an ill-defined, large, and prominent nævus, over which the skin was everywhere discoloured. By electrolysis the parts were rendered absolutely flat; but the skin became white only in the centre over a space the size of a crown, although, in this instance, I tried what in others I have found advantageous, the painting of the surface with nitric acid, blistering fluid, iodine, and other irritants.

There is yet another mode of using electricity, which I believe will come into more general use,—the galvanic cautery. It may be used subcutaneously with perfect ease. The platinum wire is placed in position in the growth, and a hollow needle is threaded on each end, pushed along it through the skin, and then connected with the battery. The motions imparted to the platinum wire will of course determine the amount of tissue cauterized. The contractile properties of the scar of a burn render it unnecessary to produce extensive destruction, and it would be advisable rather to trust to a repetition than run the risk of producing a slough. I have applied the plan only once to a nævus about the size of a pigeon's egg on the shoulder, and although I made a very limited use of the cautery, the growth afterwards gradually withered. I believe that, but for the cumbrous and costly apparatus required, it would sup-plant the seton and subcutaneous ligature, because it is not attended by suppuration.

There are three methods of treating nævi which necessarily leave a scar—excision, ligature, and ablation. They have this in common, that the cure is certain.

The danger of bleeding when excision is performed was at one time much exaggerated. It is now well known that in almost every case nævi are encapsuled. The capsule is doubtless formed, as in other tumours, by the pushing aside of surrounding textures during the growth of the adventitious structure. But, as in other tumours even of simple character, a loose capsule cannot be relied upon. Under pressure or other form of irritation the distinction between the growth and its surroundings is lost in adhesions. This gives rise to a difficulty in dissection, which is usually of little moment, but which is a source of danger in the neighbourhood of important parts. Mr Teale's operations, doubtless, show the possibility of excision in most unpromising situations, but such operations are more formidable than the disease warrants, and in many respects are inferior to other measures. Excision is inapplicable in the face, because it leaves a scar, and because it does not cure with certainty the nævoid skin. It has disadvantages in the body, because there is a certain amount of bleeding at the time, and possibly some risk of reactionary and secondary hæmorrhage. It ought, I think, to be restricted to nævi which are subcutaneous, which are situated on a part of the body usually covered, and which are not of large size. For these, it is rapid, clean, and certain.

Ligature, on account of its safety and certainty, is justly regarded as the proper mode of treatment for nævi not on the face, and which can be completely encircled by the cord. The exceptions are those just mentioned as suitable for excision, and large nævi in which the destruction of skin would be excessive, and the scar would injure the usefulness of surrounding parts. I do not think that ablation presents any advantage over ligature. The scar is equally large, the healing is not more rapid, and the security from hæmorrhage is not so great. Still, if it be at any time necessary to remove a nævus of the tongue or inner surface of the mouth, ablation by the galvanic cautery is plainly the best procedure. In such situations, methods which depend for their efficacy on inflammation or suppuration are inapplicable, and the presence of a ligatured mass which could not be prevented from putrefying, would be singularly offensive and even injurious.

I have not thought it necessary to detail, merely for the purpose of condemning, the thousand-and-one remedies which have been applied to nævus. Besides those I have mentioned, I do not think there are any of practical importance. We may have to vary our treatment according to the circumstances of the case, but our choice is really limited to a few measures, of which ligature, injection, and galvanism are the most prominent.

ARTICLE VII.—*On the External Uses of the Hydrate of Chloral.* By WILLIAM CRAIG, M.D., C.M., F.R.S.E., F.R.C.S. Ed., Lecturer on Materia Medica, Edinburgh School of Medicine.

(*Read before the Medico-Chirurgical Society of Edinburgh,* 10*th November* 1875.)

CHLORAL was discovered by Liebreich in 1832, and in 1869 he found that chloral could be decomposed by caustic potash into chloroform and the formiate of potassium. This fact led him to believe that, if given internally, it would be decomposed by the alkalies of the blood, and free chloroform liberated. He imagined that chloral would thus act as a hypnotic and sedative. As the result of this *a priori* reasoning, Liebreich gave chloral internally, and found that it acted as a powerful hypnotic. Whatever difference of opinion may exist regarding the *modus operandi* of the hydrate of chloral, certain it is that it acts speedily and effectually as a sedative and hypnotic. And, consequently, the British Medical Council have wisely included it among the medicines lately added to the British Pharmacopœia. It was only, however, on account of its uses for internal purposes that it was made an officinal medicine. About the same time that chloral was added to the British Pharmacopœia, Dr Keen, Lecturer on Anatomy in the Philadelphia School of Medicine, made some interesting experiments with this substance, and clearly proved that, in addition to its hypnotic effects, chloral was a powerful antiseptic, and, as such, might be used for a variety of purposes. Dr Keen found that a watery solution of the hydrate of chloral formed an excellent antiseptic fluid with which to inject bodies for the dissecting-rooms, and was a good preservative fluid for anatomical specimens. He also tried it as a dressing to wounds, and found it answered exceedingly well.

These remarkable effects ascribed to this substance were sufficiently interesting to warrant me making a series of experiments and observations to ascertain the correctness of Dr Keen's views. I accordingly made, during the past year, a considerable number of experiments with the hydrate of chloral, and I have now the honour of communicating the results to this Society.

I. *Solution of Chloral as a Preservative Fluid for injecting Bodies for the Dissecting-room.*

During the session 1874-75, through the kindness of Professor Turner, several bodies were injected with solutions of the hydrate of chloral, and afterwards placed in the dissecting-rooms of the University of Edinburgh. The injections were made by Mr Stirling, Assistant-Keeper of the Anatomical Museum, and the bodies were carefully watched during the whole time that they remained in the dissecting-rooms by Drs Russell and Cunningham, Demonstrators of Anatomy. About 90 grains of the hydrate of chloral, dissolved in water, were injected into each body. The results observed were much the same in all the bodies. The subjects injected with chloral were as well preserved from putrefaction as were those injected with the ordinary preserving fluids, and when the parts were protected from the air, the colour of the parts was remarkably well preserved. This was specially true of the skin. The muscles were decidedly softer than usual, but the nerves were more easily dissected out. In bodies injected with chloral, the delicate plexuses of nerves could be more easily and more successfully dissected out than when injected with other preservative fluids, partly owing to the fact that the muscles and other tissues were softer, and consequently the nerves could be more easily dissected out. There are in the Anatomical Museum of the Edinburgh University some pretty dissections of delicate nerve-plexuses, made by Dr Cunningham in the University dissecting-rooms during the last winter session from bodies injected with the hydrate of chloral. When the bodies injected with chloral were carelessly exposed to the air, they became much darker in colour. Bodies injected with chloral gave off a peculiar mawkish odour, which was rather disagreeable to the students. Several students, at my suggestion, used a watery solution of chloral (5 grains to the ounce) as a preservative fluid for their "parts," and told me that it answered remarkably well. From these experiments the following conclusions may be drawn:—

1. Bodies injected with the hydrate of chloral are preserved from decay equally well as when the ordinary preservative fluids are used.
2. If exposed to the air, or carelessly attended to by the students, the tissues become very black, and give off a disagreeable mawkish odour.
3. Delicate nerve-plexuses can be more successfully dissected out when the bodies are injected with chloral solution; and,
4. It is much cheaper than other preservative fluids.

II. *Solution of Chloral as a Preservative Fluid for Anatomical Specimens.*

The hydrate of chloral dissolved in water forms an excellent fluid in which to preserve animal tissues. In this respect its virtues as an antiseptic are well marked. I have the honour of showing to the members of this Society several specimens of animal tissues preserved in solutions of the hydrate of chloral, of the strength of 5 grains of chloral to the ounce of water; and in many cases they seem to be preserved better than when put up in spirit. Amongst other preparations—

1. I show a wen which I removed from the head of a patient in August 1874, and which is preserved in an ounce of water containing 5 grains of the hydrate of chloral dissolved in it. The specimen is well preserved.

2. I show also a small fœtus, which you will perceive is beautifully preserved. It has been kept for four months.

3. I next show the heart, lungs, liver, kidneys, and other parts of the abdominal viscera of a rabbit, preserved in the same fluid. These were put up in December 1874.

4. I also show three minnows, preserved in a chloral solution. They have now been fully nine months in the bottle, and you will see that they are much better preserved than would have been the case had they been put up in spirit.

I may also mention, that in the Anatomical Museum of the University of this city there are some beautiful preparations put up in chloral solution by Mr Stirling, and which he will be happy to show to any member of this Society who may wish to see them.

He has three salmon parr, preserved in a solution of chloral (15 grains to the ounce of water); these have been in the jar for four months, and nothing can exceed the beauty of these specimens. They are well preserved, and the colour of the skin is still perfect.

Mr Stirling has also preserved in chloral solution the stomach of a fœtal calf. This is an interesting preparation, inasmuch as the stomach is still filled with its own original fluid, and has some of the fœtal membranes attached. The whole are well preserved. They were put up in March 1875. He has also the injected heart of the same fœtus. This was put up at the same time. At first the fluid appeared to act on the colouring matter of the injection, but this was only to a very limited extent, as the preparation is in an excellent state of preservation.

Solutions of the hydrate of a chloral do not coagulate the albumen of the tissues, and, consequently, delicate transparent membranes are better preserved by chloral than by spirit or carbolic acid. There can be no doubt but, as a preservative fluid for anatomical specimens, it is superior to spirit, if not to every known antiseptic. It is much cheaper than methylated spirit, and is destitute of the disagreeable odour of that fluid.

III. *Chloral as a Dressing for Wounds and Ulcers.*

I have tried chloral extensively as an external application to wounds and abraded surfaces. I found as the result of these experiments that a lotion containing from 5 to 15 grains of the hydrate of chloral to the ounce of water formed an excellent dressing to ulcers and wounds, dressed with lint and gutta-percha in the ordinary manner. I could relate several cases, but I will select only one. A young lad, T. M., lately one of the boys in the Mars training-ship, had one of his legs severely burned, and after being treated by the surgeon in charge of the ship for three months, the boy was recommended to go into Dundee hospital to have the limb amputated below the knee. As T. M. was within a fortnight of receiving his discharge from the ship, he was allowed to visit his friends in Edinburgh, who were unwilling that the leg should be amputated, and put the boy under my care. I saw him in April last, and found a large ulcer on the leg, extending from a little below the knee to the middle of the foot, and several inches in breadth. The edges were very irregular, and a considerable amount of fœtid discharge came from the ulcer. The boy and his friends were very anxious that I would try and preserve the limb. I ordered a lotion containing 15 grains of the hydrate of chloral to the ounce of water, some chloral lint, such as is manufactured by J. F. Macfarlan and Co. of this city, as recommended by Dr P. H. Watson, and some gutta-percha, and gave instructions for having the limb dressed twice daily with these. I may mention that previously it had been dressed with some preparation of carbolic acid. After the chloral dressing the limb healed rapidly. The ulcer got gradually less, the fœtid discharge disappeared; and when I saw the patient in July last, only a small ulcer remained, and even that was gradually diminishing in size, notwithstanding the fact that the boy was daily employed in a large drapery shop in town, and was unable to give the leg that rest which was necessary, and which I had so much recommended. The boy is now a sailor, and is at present on a voyage to Athens. I also used chloral solution as an injection into the sacs of large abscesses, and found that it tended much to diminish secretion and make the parts heal. I found it also a useful lotion for the eye in inflammatory conditions of that organ. It is an excellent application to burns, and very specially where there is a fœtid discharge. I also found it a good application to remove warts from the hands and fingers. I used for this purpose a lotion containing 15 to 20 grains to the ounce of water, applied by means of lint and gutta-percha. It causes no pain, and the wart speedily becomes smaller, and gradually disappears.

I also used it as a lotion to sore nipples and to inflamed mucous membranes. When chloral is applied to an ulcer, a wound, or to the interior of an abscess sac, it causes at first some smarting, but that only lasts for a few minutes, and is soon succeeded by a most agreeable sensation. Patients so treated have frequently told me that soon after the lotion was applied a very agreeable soothing effect was felt in the wound. I believe that in all such cases chloral acts as a local sedative. It produces anæsthesia of the nerves of the part. Wherever there is a wound or ulcer there is irritability of the nerves of that part; and chloral, by soothing this irritability of the nerves, favours the healing process.

I have frequently used with good effect an ointment containing 30 to 60 grains of the hydrate of chloral to the ounce in eczema and other allied affections. I believe it to be one of the best applications in such diseases; and a medical practitioner lately told me that he had used it with marked benefit as a local application during an attack of erysipelas of the head. Chloral in various forms has been extensively used in the Royal Infirmary of this city by Dr P. H. Watson, senior surgeon to that institution, and I have the honour of appending a letter from him giving an account of his experience of hydrate of chloral as a dressing to wounds:—

"I have in my wards made use of the chloral hydrate for fully six months, and find it quite as active as an antiseptic as carbolic acid or boracic acid. It approaches nearer to carbolic acid in its effects than to the boracic acid, especially in that it is volatile, and thus by its vapour penetrates and surrounds parts to which as a dressing it has been applied with an atmosphere of itself.

"It has a marked advantage over carbolic acid, in so far that its odour is pleasant, resembling some of the ethereal compounds employed for flavouring purposes. It also is absorbed, and, in being so, deadens pain after an operation.

"I employ it in *four* forms:—

"1. A lotion of 5 to 40 per cent. in water, for cleansing away discharges around a wound, cleansing sponges used in operations, and analogous purposes.

"2. An ointment composed of concrete paraffin, white wax (Scotch), and almond-oil, to which 1-12th to 1-8th of chloral is added, while the other ingredients are liquefied by heat. The components of the ointment should at once be rubbed together, covered, to prevent the evaporation of the chloral, and cooled to a concrete form as rapidly as may be. It is afterwards rubbed up with a few drops of the solution of chloral to disintegrate it, and prevent its crystalline form being reassumed. This ointment takes great pains to make efficiently. If not properly prepared, it is either inert, containing sometimes, I find, absolutely no chloral; at other times (if made cold) the chloral is so imperfectly mingled that it acts as an irritant, and blisters tender cutaneous surfaces. The ointment is applied spread into the substance of linen cloth, so as to be incorporated with the material. This dressing forms the immediate application to the surface around the wound, and covers in the wound itself. It does not adhere, but peels off like a thin layer of wax.

"3. An external excipient dressing is made by soaking lint in a solution of chloral (ʒi. ad ℥j.). It is then wrung out of this and carefully dried. The care is necessary to avoid long exposure or a high temperature, as this volatilizes the chloral.

"4. Lint soaked in a solution of chloral in olive-oil (1–8), employed to fill cavities such as those left in some excisions, and to employ as compresses when it is desired to prevent bleeding from the cut surfaces in operations for the removal of *dead* bone.

"In some cases, when the chloral appears to act as an irritant, even when carefully prepared, it may be necessary to interpose some impermeable material between the line of operation and the dressing.

"I have never met with any disagreeable results from the absorption of the chloral. On the contrary, I have found the pain of recent wounds only satisfactorily modified and relieved by its employment."

ARTICLE VIII.—*Case of Menière's Disease.* By BYROM BRAMWELL, M.B., Physician and Pathologist to the Newcastle-upon-Tyne Infirmary.

(*Read before the Northumberland and Durham Medical Society, 11th November* 1875.)

IN the year 1861, a French surgeon, Menière, described a series of cases characterized by the following symptoms: vomiting, deafness, noise in the ears, pallor, a reeling gait; and he showed by post-mortem demonstration, that the symptoms did not depend upon disease of the central nervous system, as was formerly supposed, but upon structural lesions in the internal ear.

In some cases the onset was so severe as to resemble apoplexy. He mentions the case of a girl who caught cold whilst menstruating, was seized with deafness, giddiness, and vomiting, and died in five days. After death the only lesion found was a kind of bloody exudation into the semcircular canals and vestibule.

Since the appearance of Menière's original paper similar cases have been described by independent observers. The result has been to confirm his statements, and to show that the lesion is situated, as he supposed, in the semicircular canals.

The subject has also been worked at physiologically, and although its exact pathology is still unsettled and obscure, yet most authorities are agreed as to the main fact, that injuries and disease of the internal ear, more especially of the semicircular canals, do cause the symptoms I have described above.

It is difficult to explain the mode in which such lesions cause giddiness, reeling, and the whirling sensation which patients suffering from the disease complain of.

Goltz thinks the auditory nerve consists of two divisions: one, distributed to the cochlea, is the nerve of the special sense of hearing; the other, distributed to the semicircular canals, is not auditory, but is subservient to the maintenance of poise. The semicircular canals, he thinks, are the organs of the sense of equilibrium of the head, and therefore of the body. They contain a fluid called the endolymph; the pressure of this fluid is the balancing power. The pressure would be greatest at the most dependent point, and as this point must necessarily vary with every oscillation in the position of the head, a knowledge would be thus conveyed to the sensorium of the movements and of the new position. When the canals are opened and the fluid escapes, this knowledge would not of course be afforded.

Goltz believes that the semicircular canals are alone affected.

Crum Brown agrees with Goltz that the pressure of the endolymph is the balancing power. He thinks that the ampullæ or dilated ends of the semicircular canals are the parts where impressions are received, and points out the fact that each canal has an ampulla at one end only; there is thus a physical difference between the rotation with the ampulla first, and the rotation with the ampulla last.

In support of this view, is the fact that the nervous filaments are not distributed to the whole of the membranous semicircular canals, but only to the ampulla.

Knapp, an American, who has written an elaborate paper on the subject, is of opinion that the cochlea is also implicated, and in support of this view he mentions the fact, that in some cases there is deafness for certain groups of musical sounds only; it being admitted by most physiologists that the perception of musical sounds depends on the cochlea.

Loewenberg believes the semicircular canals are affected, and he looks on the disturbance of motion as due, not to paralysis, but to irritation of the auditory nerve acting reflexly.

The lesion of the semicircular canals may be primary or secondary. If the latter, it very generally results from some fault in the more external parts of the auditory apparatus. Sometimes it is secondary to purulent processes in the middle ear. In such cases, there may be a discharge from the external auditory meatus. In other cases, the portio dura is involved in its passage through the temporal bone. Facial paralysis on the same side as the lesion may then result.

These events are, however, simply complications, and are not necessarily symptoms of Menière's disease.

The diagnosis is in some cases difficult; especially is this the case when the onset is sudden. On this point Trölsch says, "If a person who has formerly heard well becomes suddenly deaf, or hard of hearing, with the symptoms of an apoplectic attack, and there is at the same time an uncertain or staggering gait, but there are no symptoms of paralysis in other nerve tracts, and if the examination shows a normal membrana tympani, and perfectly permeable Eustachian tube, we may believe *with great probability* that there is an affection of the labyrinth."

I have thus briefly gone over the leading features of the disease, quoting somewhat largely from an article of Dr Hughlings Jackson.[1] I will now detail the history of a typical case which I have lately had under observation.

John Moor, æt. 45, a cobbler, was admitted under my care on 15th September 1875, suffering from labyrinthine vertigo, or Menière's disease.

Previous History.—With the exception of occasional headaches, he enjoyed good health until his present illness commenced. When three years of age, his left ear "gathered," and he has been slightly deaf on that side ever since. He has not had syphilis. Has been very steady. Has worked hard in a small confined room, as he has a large family to support.

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[1] Medical Times and Gazette.

At 2 P.M. on Sunday, 30th August, he was seized with a sudden pain in his left ear (he had been dull and heavy, and had felt a slight pain in the head for two days previously). The pain rapidly got worse, "and became so severe that he could have screamed with it." After it had lasted for two hours he heard a loud click, the ear had burst, and a quantity of offensive-smelling matter escaped.

After the bursting of the abscess, the pain was much relieved, but he now heard a "fearful noise in his head; it exactly resembled a steamer blowing off steam close to his ear." He became very giddy, and felt as if everything was whirling round him. The whirling sensation was most painful, and made him sick. He was obliged to keep his eyes shut, and remained in bed with his eyes firmly closed for three days.

He vomited three times on Sunday, and several times since.

On Wednesday, 2d September, he got out of bed, "*felt just as if he were drunk,*" *reeled about, and thought he would fall to the right side.* On Friday, 4th September, he was unable to close the left eye. He noticed that he spat crooked, and that fluids ran out of the left side of his mouth.

The Family History.—Unimportant.

Present Condition.—Patient is a well-built muscular man, dark complexioned, but pale. The left side of the face is paralyzed, the left eyelid is fixed, and the patient does not wink. Sensibility in the left side of the face is somewhat impaired. Both common and special sensation in the left side of the tongue are impaired. The tip of the organ when protruded is apparently slightly turned to the right side. The muscles of the left side of the face respond less strongly to the Faradaic current than those of the right.

There is no paralysis of any other part. The patient walks with a reeling gait. When walking he keeps his eyes firmly fixed on the ground a few feet in front of him. The reeling is much worse when the eyes are shut. When walking he feels as if the feet were being lifted up of themselves. He feels as if he would tumble to the right side.

He sees everything rapidly *whirling round him from left to right.* When he turns his head to the *left* side, objects seem to glance past him with the rapidity of lightning. When the head is turned to the right side, no such sensation is perceived. He does not now feel giddy when lying quiet in bed.

There is a discharge of offensive brown-coloured matter from the left ear. On otoscopic examination the membrana tympani is seen to be pierced by a triangular slit-like opening. The external parts of the auditory canal are inflamed, swollen, and bleed with the slightest pressure. The patient is deaf on the left side, *both to external and to skull sounds*, as heard through the tuning-fork. Hearing on the right side is normal.

He complains of a constant loud noise inside his head. It is just like the blowing off of steam. He thinks he hears it in his *right* ear.

There is pain and tenderness over the left mastoid process. He has no pain in the head itself, but feels as if the left half of it were empty.

The left pupil is slightly larger than the right. Sight, if it were not for the whirling sensation, is natural. On ophthalmoscopic examination the discs are normal.

Smell is natural. He cannot breathe freely through the left nostril.

The reflex functions are normal. He sleeps badly because of the pain in his ear. He is very intelligent, and spontaneously described to me most of the symptoms detailed above.

The tongue is clean and moist; appetite poor; bowels costive. He has not vomited for several days.

The radial pulse is 84.

The temperature is normal.

Treatment.—To have the bowels freely moved; to have the left ear syringed out twice a day; and to take ten grains of iodide of potassium three times a day.

18*th September.*—He thinks he is worse. The pain was very severe last night. To-day he feels pain in the teeth of the lower jaw. To have a subcutaneous injection of morphia at bedtime, and to be blistered behind the left ear.

19*th September.*—Is much easier.

24*th September.*—Very much better. Is not nearly so giddy. Muscular power is beginning to return in the left side of the face. The tip of the tongue is no longer turned to the right side. Sensibility, both common and special, is now equal in the two sides of the organ.

11*th October.*—Greatly improved. The facial paralysis is now almost gone. He walks without any reel, but still feels giddy. He is as deaf as ever, and still hears the noise in his head.

15*th October.*—Was made an out-patient, as he had to go home to attend on his wife, who is ill.

4*th November.*—Still improving.

Remarks.—This case presented all the typical symptoms of Menière's disease. It was complicated with facial paralysis, and a discharge from the left ear.

The points of special interest are:—

1st. The fact that the patient was deaf on the left side *to all sounds.* The tuning-fork, when placed on the head, was unheard in the left ear, even when the external auditory meatus was closed by the finger.

2d. The direction in which objects whirled round him. This was from left to right, that is, from the affected to the sound side. The fact that objects seemed to fly past him, when he turned his head to the left side, was also interesting.

3d. When walking he had a tendency to fall to the right side.

The lesion of the semicircular canals was probably secondary to purulent inflammation in the middle ear.

The facial paralysis was no doubt due to complication of the portio dura in its passage through the temporal bone. The difference in the size of the pupils was perhaps in favour of some intracranial mischief.

I have now lost sight of the patient, but the result of the case, so far as it has gone, is highly satisfactory.

ARTICLE IX.—*Sclerosis in Scattered Patches.* By Prof. CHARCOT, La Salpêtrière, Paris.[1] Translated by THOMAS OLIVER, M.B., Preston, with M. Charcot's permission.

PART I.—*Pathological Anatomy.*

Summary.—History of Sclerosis in Scattered Patches—French Period—German Period—Recent French Researches.

General Pathological Anatomy.—External Appearances of the Sclerous Patches: Their Distribution—Brain: Cerebellum, Protuberance, Bulb, Spinal Cord—Sclerous Patches on Nerves—Forms: Spinal, Cephalic, or Bulbar—Cerebro-Spinal—Characters of the Patches—Colour, Consistence, etc.

Microscopic Anatomy.—Remarks upon the Normal Histology of the Spinal Cord—Nerve-Tubes—Neuroglia: Its Distribution—Cortical Layer of the Reticulum—Characters of the Neuroglia—Influence of Chromic Acid—Arterial Capillaries.

Histological Characters of the Sclerous Patches.—Transverse Sections—Peripheric Zone—Zone of Transition—Central Part.

Longitudinal Sections.—Alterations of the Vessels—Examination of Sclerous Patches in the Fresh State—Histological Lesions consequent upon Section of the Nerves—Fatty Granulations on the Sections of Sclerous Patches in the Fresh State—Modifications in the Nerve-Cells—Mode of Succession of the Lesions.

WE find sclerosis in patches mentioned for the first time by M. Cruveilhier in his "Atlas of Pathological Anatomy" (1835–42), an admirable book, and one which ought to be consulted oftener by those who wish to avoid the disenchantment of slow and second-hand discoveries in Pathological Anatomy. It is in the 22d and 23d lectures that you will see the lesions of sclerosis in patches figured. By their side you can read the clinical observations to which they refer. I take advantage of this opportunity to recommend to you the reading of a remarkable chapter on Paraplegias. Before this period, in no other work, to my knowledge, is there any mention of sclerosis in patches.

After M. Cruveilhier, Carswell, in the article Atrophy in his

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[1] Leçons sur les Maladies du Système Nerveux faites à la Salpêtrière. Par J. M. Charcot, Professeur à la Faculté de Médecine de Paris, etc. Première edition, 1872–73.

"Atlas" (1838), has made drawings of the lesions which refer to sclerosis in patches. But this author, who has drawn in great part the materials of his work from the hospitals of Paris, makes no reference therein to any clinical fact. Even at the present day, I do not believe that sclerosis in patches is known in England. I do not find it mentioned in any of the classical works published in that country, any more than in the valuable contributions of Dr Gull.[1]

Thus, up to that time, the principal documents had been collected in France. Starting from this epoch, during a period of several years, this question seems almost entirely to have been forgotten; and it is to Germany that we must now turn our attention. Ludwig Türck has published, in 1855, examples of lesions, evidently referring to sclerosis in patches; yet it was only the physiological aspect that struck him. Rokitansky indicates them in his treatise; Ferichs and Valentiner report on two observations; Rindfleisch, Leyden, and Zenker, furnish in their turn some elements for the solution of the problem. *Desiderata* remained to be filled up, and so new researches were indispensable. It is at the Salpêtrière that sclerosis in patches again attracted attention amongst us. After 1862, M. Vulpian and myself have accurately established examples of it. M. Bouchard, relying upon facts collected at the Salpêtrière, returned to this subject in a paper read at the Medical Congress at Lyons.

In the preceding enumeration, we have particularly taken into account works having reference to pathological anatomy, proposing to ourselves to speak subsequently, and more at length, upon those which contain clinical details. To the information given us by the above-mentioned authors, we will add from other observations as yet unpublished, and, to facilitate the comprehension of our studies, we shall place before you the anatomical sections which we have preserved.

General Anatomy.

Sclerosis in scattered patches, is, as I have told you, not an affection exclusively spinal. It invades the brain, the protuberance, the cerebellum, the bulb, as well as the spinal cord. We shall now enumerate the alterations which we discover, in the most marked cases, upon these various segments of the nervous system—first externally, and then upon the sections.

The question before us is an alteration relatively coarse, and it is surprising that it has existed such a time without being noticed. Upon the plates which I show you, and in which the alterations are faithfully reproduced, you see the spinal cord spotted with grey patches, of a more or less irregular outline, but in every case clearly circumscribed, and which sharply define themselves from adjoining structures.

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[1] The Editor warmly recommends the reading of an able article on "Insular Sclerosis," by Dr Moxon, in Guy's Hospital Reports, vol. xx. 1875.—T. O.

Sometimes isolated, sometimes confluent, these patches or spots, as you can easily ascertain, are disseminated without any apparent rule, and as if at random, over all points of the cord. The bulb itself is not spared, far from it. Often various parts of the encephalon are also affected.

But we cannot confine ourselves to this simple sketch—we must enter into the details of a more regular description. In the first place, we must say that *a purely external examination* would give only a very incomplete idea of the lesion. The patches, the spots of which we have just spoken, are not superficial; they constitute true nuclei or foci, which penetrate into the depth of the tissues. Often section alone reveals the existence of patches hidden in the interior.

Let us examine in the first place the *brain.* The general aspect of the *brain,* properly so called, has not undergone any modification in its form, and we can also add in its colour, for the patches are very rarely met with on the grey substance of the convolutions. It is not the same with what concerns the central parts. In fact, we find patches, especially upon the walls of the ventricles, in the white substance of the centrum ovale, the septum lucidum, corpus callosum, and, in fine, in certain parts of the grey substance (optic thalami, corpora striata).

The *cerebellum,* from its constitution, usually presents only internal patches, which occupy specially the rhomboidal body.

The *bulb,* the *protuberance,* and the various surroundings of the isthmus, are very frequently the seat of sclerous patches, and there they are at once peripheric and deep. Upon the bulb the patches affect in an isolated way, or simultaneously, the olivary bodies, the pyramids, the restiform bodies, and the posterior region, in which are placed, above each other, the origin of the bulbar nerves. As to what has reference to the protuberance, the patches are generally placed on the antero-inferior face. If we go still higher, we see that both the mammillary tubercles and the cerebral peduncles are affected.

We come now to the *spinal cord.* Through the pia mater we often perceive grey spots, which assume a rosy tint, similar to that of the flesh of salmon, when exposed to the air. But it is principally after the removal of this membrane (a removal which is effected without difficulty) that we observe clearly the lesions. They affect all parts of the spinal cord, cervical, dorsal, and lumbar; they invade indistinctly the different bundles, without respecting the furrows; and they proceed equally upon the grey as upon the white substance.

The *nerves* themselves do not escape the sclerosis. We see them sometimes issue, at their origin, from a sclerous patch, and show themselves perfectly sound. At other times we find on their course sclerous patches in every respect similar to those of the nervous centres, at least upon the portions of these nerves adjacent to the centres. The observations of MM. Vulpian and Liouville, often repeated since, leave no doubt on this point. The *cranial* nerves which have presented sclerous patches are the optic, olfactory, and fifth pair of nerves. As for the spinal nerves, we know that patches have been seen upon the posterior and anterior roots, but we do not know whether they have been injured in their extra-spinal course.

I will not dwell longer on the topography of sclerous patches; nevertheless, I cannot avoid drawing your attention to the interest which is attached to it.

You see, in fact, that the patches fix on very different regions of the nerve centres, according to the cases; and it is clear, that to these varieties of position must correspond very different functional derangements. It is to this that the disease owes in great part its Proteus-like character. We will return to this point. For the present, observe that the differences of position give rise to important divisions, which we shall find again in the clinique. Sometimes the patches occupy exclusively the spinal cord (*spinal form*); sometimes they predominate in the brain (*cephalic* or *bulbar form*); in short, the simultaneous existence of patches in the brain and cord corresponds to the *cerebro-spinal form.*

To conclude the general anatomy, nothing further remains than to point out the principal characters which the patches considered individually present.

Sometimes the patches are elevated, and, as it were, turgescent; at other times they are on a level with the neighbouring parts. Lastly, they are sometimes depressed, when they are of old standing.

They have a coloration resembling closely that of the grey substance, from which it is difficult to distinguish them; but when in contact with the air they take on a roseate hue, and we see abundant vessels delineated in them.

These patches are of firm consistence, and give clean sectional surfaces, from which there flows a transparent liquid.

Such is, in point of view of the simple anatomy, sclerosis in generalized patches. It is now necessary for us to enter into minute histological details.

To lead to a satisfactory termination, this undertaking, which has reference to facts requiring laborious exposition, I shall claim at once both all your attention and all your indulgence.

Microscopic Anatomy.

The method to follow is simple. We must set out from normal conditions. These once known, it will be easier to derive from them a knowledge of morbid conditions. Previous acquaintance with the characters of the normal state, in what concerns the organs and elements of which we wish to study the degenerations, is without doubt familiar to you, and we could strictly enter offhand into the examination of intimate lesions. Yet, as you know, the histological anatomy of the nervous centres is in some respects quite new. A great number of the questions which it raises are still undecided; nevertheless, for the understanding of pathological lesions, it is not a matter of indifference to entertain towards these questions an opinion more or less well grounded. These considerations induce us to recall to your memory, at least summarily, certain fundamental facts of normal anatomy. Besides, we shall be occupied chiefly with the spinal cord, an organ less complex, and of easier access than the brain. But in order to limit the field of our studies, we will not stop to describe the nervous elements, properly so called, tubes or cells, nor will we enlarge upon their reciprocal relations, nor upon the mode of grouping which they affect in order to constitute what is called the white and grey substance. We propose to concentrate your attention upon the conjunctive envelope, which surrounds these elements on every side. A great interest is attached to the history of this conjunctive sheath, principally for the pathologist; for it is to it that we must attribute the chief part in certain degenerations of the nervous centres, and, in particular, in the cases which occupy us.

I.—(*A.*) It will be useful, I think, to begin this study by examination of thin transparent slices, cut transversely from pieces of the spinal cord, properly hardened in a solution of chromic acid, and coloured by carmine. The carmine here is a valuable reagent. Thanks to it, certain elements which have the property of assuming under its influence a more or less lively tint, are by it brought into relief, at the same time that the others preserve their usual aspect. In this way the ganglionic cells, their nucleus, their nucleolus, and also the prolongations of these cells, are strongly coloured by this reagent. The conjunctive sheath is coloured equally in all points of its extent, but in reality in a much less decided manner; and, as to what refers to the nerve tubes, only the axis cylinder takes the colour of the carmine, whilst the envelope of myeline completely resists its action.

All the details which this preparatory mode brings into prominence, you can follow upon the plate taken from Deiters, which I show to you. Afterwards, you will again easily find them in the very beautiful sections which I shall pass before you, and which I owe to the courtesy of our confrère M. Lockhart Clarke. We shall in the first place examine these pieces by the aid of a feeble magnifying power.

Upon the preparations, as upon the plates, the parts which belong to the white substance of the cord appear to you without doubt, at first sight, almost entirely composed of little bodies regularly rounded, kind of discs placed side by side, and pretty nearly of the same diameter. These are very thin cylindrical fragments resulting from section of the nerve tubes, which tubes are, in this part of the spinal cord, arranged for the most part along the great axis of the organ, and are, just as the prisms of a basaltic causeway, parallel to each other. In the centre of the discs, which, in the rest of their extent, are constituted by the uncoloured myeline of brilliant and translucent aspect, there figures like a point, or better, like a little globule, the axis cylinder coloured red.

A closer examination soon establishes the fact, that the discs in question are not exactly contiguous, and that they are, on the contrary, more or less clearly separated from one another by a substance of homogeneous appearance, which the carmine colours lightly, and which seems to fill up, in the manner of a cement, all the gaps which the nervous elements leave between them. This substance is no other than the conjunctive sheath, as we called it a little while ago, or, as it is otherwise called, the neuroglia (Virchow), or the reticulum (Kölliker). In studying its mode of distribution and arrangement upon the various parts of the section, you will easily recognise that it enters as a very important part into the mass of the organ. Notice, in the first place, that it forms, in the circumferential part of the section, a ring, or better, a zone, of a certain thickness, and in which the nerve tubes wholly disappear. This zone is covered externally, and enveloped, so to speak, by the pia mater, with which it forms only feeble adhesions; it is besides perfectly distinct, as to its structure, from this latter membrane, which is composed of fiibrillar conjunctive tissue, and, consequently, quite otherwise than the neuroglia. It has been described with care by Bidder and by Frommann, who designate it under the name of *cortical layer* of the reticulum (Rindenschicht); we shall see later on that it presents now and then, in a pathological point of view, an unquestionable interest.

From the internal border of this zone, or cortical layer, we see partitions arise, which become separated from each other at equal distances, and are directed towards the centre of the cord, which they divide into triangular compartments nearly equal, whose base is at the periphery, and whose summit loses itself in the grey substance. These partitions themselves give birth as they proceed to secondary and tertiary tractuses, which are also subdivided in their turn. Their ramifications are interwoven, crossed, and anastomosed in such a manner as to produce a network of meshes of unequal dimension. Of these meshes the widest collect together under the form of bundles, eight or ten nerve tubes, or even a greater number, whilst the narrowest enclose most frequently only one of them. The reticulated arrangement in question becomes especially evident at those points of the preparation, in which, in consequence of the disappearance of the nerve tubes, the conjunctive skeleton alone persists.

Perhaps still more than in the white substance, the neuroglia plays in the grey substance an important part. It is, in short, from regions of this latter that it is formed in an almost exclusive manner; such are, for example, the borders of the central canal, the ependyma. It predominates also in that part of the posterior cornua known under the name of substantia gelatinosa of Rolando; in the posterior commissure, which in consequence assumes in almost its whole extent a roseate tint in the preparations treated by carmine, whilst the anterior commissure, on the contrary, by reason of the numerous nerve tubes which it contains of transverse direction, is much less affected by the reagent. In the grey substance, just as in the white, the neuroglia presents the reticulated structure; only in the first case, the much more multiplied intricacies of the trabeculæ form meshes notably more thickly set, and exhibit the appearance of a spongy tissue. In these two conditions, moreover, it serves as a support to the blood-vessels.

(*B.*) It is proper now to examine by the aid of more powerful magnifying glasses what is the histological constitution of this conjunctive sheath of which we know only the most outward appearances. Is it ordinary conjunctive tissue (laminous, fibrillar tissue)? No, assuredly; every one is agreed upon this point. But beyond this notion, purely negative, nearly everything remains disputable in the histological history of the neuroglia. Yet, here an opinion tends to prevail, and this opinion, if I judge of it in accordance with impressions founded on personal observations, would come near the reality. According to this manner of viewing it, the neuroglia would be formed like the stroma of lymphatic glands, for example, following the type of *simple reticulated conjunctive tissue* (Kölliker); that is to say, it would be essentially composed of star-like cells, in general poor in protoplasm, bearing slender prolongations, several times ramified, and whose branches communicate with one another in such a way as to bind into a single system the various cells, and render them, so to speak, reciprocally dependent (Kölliker, Max. Schultze, Frommann). In this form of connective tissue, there exists only very little amorphous matter in the meshes of the reticulum, and the intermediary fibrillar substance, which is one of the fundamental characters of laminous tissue, completely disappears here.

(*To be continued.*)

FEES FOR MEDICO-LEGAL WORK.

No doubt, many of our professional readers have had occasion practically to deal with the subject indicated in our title, and have discovered for themselves, perhaps at the cost of a pecuniary loss, the best way of securing themselves for the future against a recurrence of such loss. But there may be some who have not gone through this experience; and for their benefit we supply the following brief narrative of a recent case of medical hardship, from which one or two very obvious practical lessons may be drawn. We, of course, suppress the names; but we can vouch for the accuracy of all the particulars.

The son of a rich merchant was injured in a railway accident, and the injury resulted in the amputation of a limb. The treatment following upon the accident was all conducted at a distance from the house of the patient. With this treatment the family physician, who is a well-known leading practitioner in one of our largest towns, had nothing whatever to do. He did not see the patient until his return home after he had got out of the hands of the surgeons, and there was then no occasion to prescribe for him. He was, however, visiting the house in connexion with the illness of a relative; and, being a qualified surgeon, and having formerly practised as such, he, at the request of the patient, occasionally examined the stump, simply to see that everything was going on well. But his attendance at the house ceased with the death of the patient's relative.

Some months afterwards, the patient's father called and asked the physician to assist in connexion with the approaching trial before a jury of an action for damages against the railway company, which had been raised in the patient's name. He agreed to do this, *saying nothing whatever about fees.* Accordingly, he attended at the law-agent's office, and was precognosced at considerable length; the precognition, however, being rather of the nature of a consultation on the whole medical aspects of the case than a record of facts to be spoken to as mere evidence. Of such facts the physician was necessarily acquainted with very few, and these not material to the case. He further attended two separate meetings at the patient's home, to "protect" him while being examined first by one surgeon, and a second time by two surgeons, all in the employment of the railway company. Besides this, the patient's father called at least on six separate occasions on the physician, and consulted him about points in the getting up of the case for the jury.

The trial was fixed to take place on a Tuesday in summer, and the physician was engaged, but not legally cited, to be present. With this understanding, and being in want of a day's rest, he arranged for his practice being conducted on the previous Saturday and Sunday; and on the Friday left home and joined his family, who were living in the Highlands. He had further arranged to resume his practice early on the following Monday, having left directions and messages for patients to that effect.

On the Saturday evening he received from the merchant a telegram (which was sent on upwards of six miles by a messenger), urgently requesting him to be in Edinburgh by the first train on the Monday to "protect" the patient in another examination by an eminent surgeon employed by the railway company. This request was complied with; but, as the examination did not take place early in the day, it was useless to leave Edinburgh for the night, merely to return early next morning. He accordingly remained until the trial was over on the following day. He was examined as a witness on merely one or two unimportant facts; but no special defence involving scientific questions was set up, and he was not required as a skilled witness. The trial resulted in a verdict for the pursuer for £3500.

The law-agent almost immediately sent the physician £10, 10s. as his fee; but, on their meeting casually on the street, the latter said it was too little: whereupon the law-agent said he did not know the details of what the physician had done, and asked him to render his account, that as much of it as possible might be recovered from the railway company. It was accordingly rendered as follows, only we omit the details of the figures:—

Precognition by Mr ——, ...	£0	0	0
Visit to Mr —— with Dr ——, . . .	0	0	0
Do. do. with Drs —— and ——, .	0	0	0
Travelling and other expenses going from ——shire to Edinburgh, and returning to ——, . .	0	0	0
Attendance at examination in Edinburgh, . .	0	0	0
Detention in Edinburgh, and attendance at Court, .	0	0	0
	£30	8	0

It will be observed that no charge was here made for the half-dozen meetings with the merchant.

Soon after, the physician was informed by the law-agent that the Auditor of the Court of Session had taxed his account, as against the railway company, at £3, 3s.; and the law-agent, saying, "We of course cannot offer such fees," adhered to his previous offer of £10, 10s. This fee was returned; and then the agent offered £15, 15s. This was not accepted, and thus the matter rested for some time; when the physician consulted his own lawyer. He advised that the physician could recover in Court on the simple principle of recompense for time, and trouble, and outlay, provided the fees charged could be proved to be not more than those usually charged in such cases by other medical men of similar standing. But, on other grounds, he advised against going into Court at all. A short statement of the time and trouble given was then laid by the physician before three of the most eminent surgeons and one of the leading physicians in Scotland, and they were asked to fill up a schedule corresponding to the items of the above account (only adding the merchant's calls) with what they would consider fair charges. They were consulted quite independently of, and unknown to, each other. One of them filled up the schedule with £58, 8s.; another with £51, 9s.; and a third with £49, 17s. 6d. The fourth gave his own scale for medico-legal work, below which he will not undertake such work at all, and, applying it to the schedule, the amount would be about £60.

Thus fortified, the physician's lawyer endeavoured to arrange with the merchant's agent to refer the question to a neutral lawyer or medical man of acknowledged standing. But the merchant would not refer to any one but the Auditor of the Court of Session, failing which reference, he said the matter must go into Court. As there were obvious objections to the Auditor as referee in such a matter, the physician declined to go to him as arbiter; and intimated, through his agent, that while he would not go into Court, he would not take less than his account as rendered, as he could not admit, either by taking such payment or in any other way, that his account was unjustly charged. The answer to this, received from the merchant's law-agent, was that the merchant "firmly believes that the sum of £15, 15s. is more than adequate remuneration for the trouble Dr —— had in the matter;" and that, as he had declined to receive that sum, it had been paid in equal proportions to two local charities.

Here the matter has ended; the merchant, no doubt, intending to persuade himself if possible—with what success he only knows—that in this fashion he has succeeded in paying a just debt.

But for the medical profession the story carries a very obvious moral, which may be summed up thus:—*Never trust for adequate remuneration for medico-legal work to honour, whether legal, commercial, or other honour, but, before you begin the work, make a clear bargain, in writing if possible, as to the fees you are to receive.*

It is quite true that with very many lawyers, as well as others, it is practically safe enough to trust such a question; but there are many men of excellent legal attainments who are quite unable, either from temperament or from defective general training, to take a broad and generous view of the value of professional services. They can understand the value of brains in the shape of skilfully procured or skilfully manufactured articles of commerce, which can be estimated by prices current, but not in the shape of skilled and wise advice, to be able to bestow which has cost the professional man years of arduous toil. It is to protect themselves against such well-meaning but narrow men, as well as against occasional instances of a lower type, that we recommend our professional brethren to adopt invariably the above practical rule which we have suggested.

Part Second.

REVIEWS.

Ziemssen's Cyclopædia of the Practice of Medicine. Vol. X. *Diseases of the Female Sexual Organs.* By Professor CARL SCHROEDER. Vol. V. *Diseases of the Respiratory Organs.* By Professors JUERGENSEN, HERTZ, RUEHLE, and RINDFLEISCH. London: Sampson, Low, Marston, Low, and Searl. 1875.

SCHROEDER'S name is so well known as that of a careful and accurate writer on subjects connected with the female sexual organs, that in itself it is a guarantee for the fair accuracy of any work on these subjects to which it may be prefixed. The present work is by no means exhaustive on any of the subjects of which it treats, but it is perhaps, therefore, all the more suitable as part of a cyclopædia—a general library of condensed articles—on the practice of medicine. We have looked over it with much interest, and in spite of many shortcomings, we think it would be difficult to point out any similar work better suited for the requirements of a general practitioner. The illustrations especially, without having any pretensions to artistic beauty or strict accuracy, are yet so clear and definite as admirably to illustrate and explain the letterpress. The various sections are of varying value and fulness. That on gynæcological examination is very good, perhaps the best in the book. That upon uterine malformation is also very good; while the other sections are perhaps balder and more brief than their importance deserves, though they are nevertheless about as full as the nature of the work permitted, and, being written mainly from the pathological standpoint, contain information and views which are in some respects novel to the British practitioner, to whom the clinical aspect of such cases is the more familiar one. This work is therefore very well fitted to stimulate thought and inquiry among those to whom it is now introduced, and it is therefore all the more likely to produce good fruit in the future; for the value of any medical work does not consist so much in its being up to the present day, as in its containing material for future progress.

The other volume, on the Diseases of the Respiratory Organs, is even more valuable for the same reason. It opens up to that hypothetical individual, the average general practitioner, entirely new views of how he ought to regulate his practice—not simply by what is good for this or that symptom, but by what is likely to modify the course of this or that process. The line of thought cannot fail to be productive, though what it has as yet led to may be a mistake. And in regard to pneumonia—that famous *champ de bataille*, the very mention of which brings all the veterans to the front, *auribus erectis*—there can be no doubt that Juergensen has followed a wrong lead, is off the scent, and must hark back a long way before he gets into the right line. Yet even here the line of argument is good, and cannot fail to be productive in the future. It is amusing to find Juergensen, as "an act of pious duty to the past," referring in but a few lines to "hyperinosis" and "fibrinous crasis"—matters which were of no small moment in that modern rehabilitation of the humoral pathology in which the Vienna school played so important a part, which, with all that hangs thereon, still lie beneath the crust of modern pathology, like the temporarily-extinguished fires of a slumbering volcano; for the solidism of the time immediately preceding Rokitansky's advent was not so easily swept away by the tide of modern humoralism as Juergensen's pathology will be by the advance of more modern views. Will be swept away, we have said; but it is already gone—it has no foundation. Fortunately for mankind, other and more correct views of the pathology of pneumonia have been springing up more naturally out of the old facts, which will, we hope, prevent us ever being turned back into the mischievous delusions of some of our forefathers. But in Juergensen's pathology we shall find no safeguard. He has followed Ziemssen entirely in regarding pneumonia "as a constitutional disease, not dependent upon a local cause;" that is, as something akin to typhus, to be temporized with till the system throws it off, which can never be cut short, or in any respect cured. This view, propounded by Ziemssen himself many years ago, is, however, founded upon imperfect data—data which, when more fully extended and regarded from a right point of view, do not bear the interpretation he put upon them. Ziemssen took the mortality reports of London for sixteen years, from 1840 to 1856, and, projecting their returns in the form of curves, showing the mortality from pneumonia, typhus, and other inflammatory diseases of the respiratory organs, as bronchitis, he fancied he perceived a greater similarity between the curves of typhus and pneumonia, than between those of pneumonia and other inflammatory diseases of the respiratory organs. A glance at page 29, where these curves are represented, shows upon what a slender resemblance he has based a great and important theory. Quite recently, however, Dr Arthur Mitchell and Mr Alexander Buchan, the Chairman of the Medico-Climatological Committee, and the Secretary to the Meteorological Society of Scotland, have submitted the mortality reports of London to a more searching inquiry, and have constructed annual mortality curves for all the diseases contained in these reports, based upon the returns of thirty years, beginning with 1845,—*vide Scottish Meteorological Journal* for July 1874—July 1875; and a single glance at the curves obtained by them, shows at once how erroneous Ziemssen's idea is, because, according to them, the usual curves of mortality from bronchitis, pneumonia, and asthma, are almost identical, and vary very considerably from that of typhus. We have not space to pursue the subject further here; it is sufficient for the present to point out that Ziemssen's theory is entirely erroneous; and therefore all deductions based upon it have no foundation on which to rest. In particular, the idea that we cannot cut short a pneumonia because it is a constitutional disease, falls at once with the theory upon which it rests. We by no means say that constitutional diseases cannot be cut short—that is another question—but we say that pneumonia is not a constitutional disease, but ranks itself as a local injury like bronchitis, and all surgical experience teaches us that the reaction following a local injury can be cut short; and we know of nothing in the medical history of disease to prove that the reaction following injuries to internal organs may not be modified or cut short; and to this we may also append the query, Are the lungs not rather external than internal organs? And as that would place pneumonia in the hands of the surgeons, and bring it within the range of antiseptics, we can perceive what important novelties as to treatment may possibly originate in the thirst for knowledge of even such an insignificant copartnery as the Meteorological Society of Scotland. Henceforth, those who will treat pneumonia with sitz-baths must do it at their own risk, and not because there is any real connexion between it and typhus, typhoid, or continued fever. By the way, it is amusing to find cold affusion, first in recent times strongly advocated in vain by a British physician, Currie, imported with *eclat* from Germany, just as, in what Juergensen would call past times, cod-liver oil, which in last century was largely made on our coasts, and greatly employed in our practice, was imported in small square liqueur bottles from the heart of Germany, and was actually described by a critic—would-be—as the oil of the liver of young asses, *oleum jecoris aselli.* It is, however, something to be in the van of medical progress, even though our views should have to be bolted through German sieves before being finally accepted.

The other papers in this volume are all valuable, and treat of subjects, many of which have not been popularized as yet among British medical men. We refer especially to embolic pneumonia, catarrhal pneumonia, and the pathology of tuberculosis. These are all subjects of the greatest importance, and we hail with the utmost satisfaction whatever opens up new lines of thought. The views and ideas may be wrong, as in many respects they no doubt are, but the habit of inquiry is started, and diseases are presented from a comparatively novel point of view—the pathological; and all this cannot fail to result in good to the practitioner himself in the first place, and to mankind secondarily; and though at first sight it is sufficiently repellent to read not a word about cod-liver oil in the treatment of phthisis, nor of those wonderful remedies, the hypophosphites, we still hope for great good from the popularizing of pathological views, which are undoubtedly those most likely to lead to permanent good, as showing how largely certain forms of disease are merely slow processes of dying, which cannot be cured, though the process may occasionally be protracted; thus leading us to strive more earnestly to maintain health and prevent disease, as the only true method of attaining longevity. There can be little doubt that the spread of such views among non-professionals would lead to very different habits of life, as well as very different views of our mutual relations, than those now prevalent, and would conduce, not only to increased longevity, but would even tend more to hasten the Millennium than many thousand volumes of Dr Cumming's discourses.

Medicinal Plants: Being Descriptions, with Original Figures, of the principal Plants employed in Medicine; and an Account of their Properties and Uses. By ROBERT BENTLEY, F.L.S., and HENRY TRIMEN, M.B., etc. Parts I., II., and III. London: J. and A. Churchill: 1875.

THE design of the authors is to furnish "An illustrated Botanical Guide to the British, United States, and Indian Pharmacopœias." It is their intention to give coloured illustrations of all the plants contained in the British Pharmacopœia, besides a number of the plants officinal in the United States and India.

We have before us the first three parts, and judging from these, we anticipate that this work on Medicinal Plants, when completed, will form an invaluable addition to our works on Materia Medica.

The botanical descriptions are in general correctly given, as well as their geographical distribution, with their actions and uses. Each plant is illustrated by a coloured drawing, natural size, and these are on the whole well executed. Possibly, in some instances, the green colour has too much of a "washed-out" appearance; but the illustrations tend to give a very good idea of the appearance of the plant.

It would be a decided improvement if the authors would place the *description of the plate* beside the plate at the beginning, and not, as at present, at the *end* of the description.

We highly recommend the work to all who take an interest in the Materia Medica derived from the Vegetable Kingdom.

On the Treatment of Simple Fracture of the Shaft of the Clavicle. By FRANCIS VACHER.

THIS pamphlet is reprinted from the *Liverpool and Manchester Medical and Surgical Reports.* For the treatment of this fracture Mr Vacher recommends shoulder-pieces of sheet-tin soldered at the most dependent parts, where they are strengthened by stout brass-wire. They are connected both before and behind by straps and buckles. The apparatus is essentially a surgical luxury.

Manual of Materia Medica and Therapeutics. By ALEXANDER MILNE, M.D. Third Edition, Revised, Enlarged, and Re-arranged by WILLIAM CRAIG, M.D., C.M. Edin., F.R.S.E., F.R.C.S.E., etc., etc., Lecturer on Materia Medica, Edinburgh School of Medicine.

THIS excellent little manual of Dr Milne comes out in its third edition as a work of more pretension, as revised, enlarged, and re-arranged by Dr Craig. It aims now at being a text-book for the student; and so, trying to kill two birds with one stone, misses both. It is not full enough for the advanced student or young practitioner; nor is it up to time in its therapeutics. For instance, we are gravely told, page 13, "that bromide of potassium in ovarian disease has enabled us to stay the knife of the ovariotomist, suspicious enlargements gradually softening down, and finally passing away." Such ovarian tumours must have been very suspicious indeed. Again, in describing the action of digitalis, in trying to be brief, it is somewhat obscure. It is termed a narcotico-acrid, irritant, and sedative. Would it not be much better if the students were simply told, that therapeutically it was a cardiac and vascular stimulant? and, secondarily, a diuretic in dropsy, on account of this action on the circulation, as in health its diuretic action is at most a doubtful one?

Dr Craig also insists strongly on the superior efficacy of aloin over aloes in doses of ½ to 1 grain, the dose of aloes being stated as 2 to 6 grains; but every practitioner is aware that, for most purposes, a much smaller dose is sufficient, ½ to 1 grain generally fulfilling all its indications. Hence it is very doubtful if aloin is more powerful or even equal to good Barbadoes aloes. In Part IV. are placed posological tables, which are not quite so full as those of Dr Henry, otherwise they would be more useful.

The West Riding Lunatic Asylum Medical Reports. Edited by J. CRICHTON BROWNE, M.D., F.R.S.E. Vol. V. 1875.

IN several respects this volume is better than those which went before it. Like them, however, it is largely made up of papers written by gentlemen who have no connexion with the Wakefield Asylum. Some of the contributions by these gentlemen give indications of having been prepared as the result of invitations to write, but they are nevertheless good and instructive.

Dr Ferrier writes of Menière's disease, and gives a case which appeared "to show that the semicircular canals are, as regards hearing, specially concerned in the reception and transmission of skull vibrations in contradistincton to aërial vibrations." This will be interesting if it be supported by further observations.

Dr Hughlings Jackson gives the best paper in the volume—On temporary mental disorders after epileptic paroxysms. This is a useful contribution, and shows that physicians in ordinary practice have opportunities of observing disordered mental action which few of them as yet have turned to good account.

In his paper on cerebral hyperæmia, Dr Milner Fothergill reminds us of "the true physiological cerebral hyperæmia of brain activity, contrasting with the anæmia which is an essential factor of sleep." He devotes but little space to the cerebral hyperæmia produced by drugs. In the course of his remarks, however, on that subject, he says, with apparent confidence, that "the period of excitement in the general paralysis of the insane, where there is great cerebral and vascular excitement combined, are controlled by Calabar bean." We think Dr Forthergill will not long continue to be of this opinion.

The Editor of the Reports contributes two articles. The first of these deals with the functions of the thalami optici, and attempts to establish by bed-side and after-death observations, "the intimate association of the corpus striatum with motor, and of the optic thalamus with sensory functions." That this is not an easy task, Dr Browne properly points out. The difficulty depends on "the frequency with which cerebral hæmorrhage involves several of the great ganglionic masses at the base of the encephalon, and with which successive hæmorrhages obliterate or confuse the records of early and definite lesions," making it a rare occurrence "to meet with a case in which the distinctive effects of a clot in the optic thalamus can be traced out in a thoroughly satisfactory manner." Such cases, however, have presented themselves to Dr Browne, and are here recorded and laboriously discussed.

Why Dr Browne wrote his second article—A note on chronic mania—is not clear. The volume was large enough without it, and it adds nothing to our information. It has afforded an opportunity, however, for that kind of writing in which Dr Browne is undoubtedly an adept. He may be said to write too well. Magniloquence in a medical report is not quite at home. It so happens, indeed, that it almost never presents itself in medical writings, except in those which treat of mental disorders; and it has been alleged that the fact of its occasional appearance in such writings has tended to lower the position of that branch of medicine which deals with insanity.

In this note on chronic mania one remarkable case is recorded.

It is this:—"I have had under my care," Dr Browne says, "a chronic maniac who worked energetically as a navvy all day, and who sat up in bed talking and shouting all night, for six months together. During these six months he was never known to sleep. By day he manifested not the slightest drowsiness, and by night he was never caught nodding. The night attendants visited him hourly, and at every visit he was in the same position, propped up in his bed, and at the same occupation, pouring out vociferously a torrent of incoherence." This man "had not lost weight after his six months' noisy vigil."

Medical and Surgical Uses of Electricity. By BEARD and ROCKWELL. Second Edition. London and New York: 1875.

THE recent publication of so large a number of works on the medical applications of electricity, gives proof of an increasing interest in the subject on the part of the profession. There is yet, however, a deficiency in this branch of medical literature. We have several moderately good handbooks, but none of them are at once compendious and accurate; a considerable number of works of reference, but none are at the same time clear and exhaustive. It is unfortunate that, to produce such works, there is required on the one hand an intimate acquaintance with the physics of electricity, and on the other, a medical and surgical knowledge both extensive and precise. Such a combination is rare, and the numerous works which have been produced invariably show a deficiency in one or other of these essentials.

The book before us is no exception to the rule. It is the result of the labours of men who have plainly done much to make themselves acquainted with electricity, but who are just as plainly defective in their knowledge of scientific medicine and surgery. It is a consequence of this that some portions of the work are worthy of much commendation, while others are worse than useless, because misleading and erroneous. The book naturally divides itself into two nearly equal portions. The first deals with the properties of electricity generally, and in its relation to the body in the normal condition. The second details the therapeutics of electricity.

Were it necessary to characterize the work as a whole, we should say that both departments present strongly-marked symptoms of indigestion. There is display of a great amount of research, but the crude mass has not been properly assimilated; the information given is extensive, not selected—has been sought out, but not thought out. But apart from this, the first portion of the book, for the reason already given, is infinitely better than the second. The physics of electricity is well and clearly given—more completely, indeed, than in any similar work, though not at greater length than is necessary for proper comprehension of the subject. The physiological section is also on the whole good, although, throughout, it is marred by that bane of specialists, a tendency to magnify their subject, to accept as proof of the effects of electricity what is often at best but a hint or a suggestion. The personal experience of the authors is given under this head as to the effects of electricity on nutrition. In operating upon patients, they are frequently in the habit of passing the current through their own bodies, and the effect has been that "both arms of each one of us have not only increased much in size by actual measurement, but also correspondingly in strength and hardness," and that the deltoid and biceps muscles have especially benefited by the process. We confess to a certain timidity in criticising men who have such means at their command, but we take courage from a consciousness of duty, and the fortunate interposition of the Atlantic Ocean.

It is impossible to speak so favourably of the therapeutic portion of the book. The description of apparatus is good and full, and is illustrated by many woodcuts, which, if they are not always very elegant, at least show what is meant. In other respects, there is little to praise. The chapter on the History of Therapeutics would lead one to suppose that, but for Messrs Beard and Rockwell, electricity would have been nowhere. The chapter on "General Suggestions" contains nothing that might not have been "suggested" in a few pages. The remainder, dealing with the application of electricity to special forms of disease, is mainly a record of the cases which the authors have treated. There is no pathological state to which the human body is liable, from marasmus to catarrh, which has not been subjected by them to electric currents; and it would be difficult to find anywhere a more perfect example of the faults and follies of specialism. The diagnosis of cases is sometimes manifestly wrong, constantly uncertain and vague. The treatment is often absurdly erroneous; the result, whatever it be—and it appears to be generally stated with candour—is never unfavourable to electricity: if unfortunate, the wrong current was used; if fortunate, a cure is claimed.

Messrs Beard and Rockwell claim as peculiarly their own two applications of electricity, one of which they call general electrization, the other the method of "working up the base of a tumour." The former consists in submitting to electric action simultaneously and successively the whole of the central nervous system. An idea of the value of the method is obtained when we state that by it may be cured "hysteria, insanity, neurasthenia, gastralgia, dyspepsia, eczema, psoriasis," and a host of other affections. The latter, by means of which tumours of every variety, simple or malignant, are caused to slough off, is worthy only of utter condemnation. There are certain growths which are best treated by electrolysis, because in that way we can bring about their destruction and absorption without destroying also the skin. But if a tumour ought to be removed from the body, the knife, the galvanic cautery, and the ligature are methods of doing so which this device can in no circumstances replace.

A Course of Practical Instruction in Elementary Biology. By T. H. HUXLEY, LL.D., etc.; assisted by H. N. MARTIN, B.A., M.B., etc. London: Macmillan and Co.: 1875.

THERE can be little doubt that a manual of this kind was required to supplement the zoological and general biological teaching of our day. The practical illustration of the knowledge given in systematic lectures is a *sine qua non* in modern biological instruction, and a very natural result of the advances of late years in teaching of all kinds. Yet, books, however excellently written, are, after all, very secondary matters in this practical work; and we cannot say that in any case, we should recommend the teacher to sink his individuality as an expounder and demonstrator, in the methods of this or of any other manual of like aims. The actual use of such works to the student, in directing his own microscopic and other studies at home, has, we are convinced, been greatly over-estimated. The plain directions given in the book as to *what* is to be seen, and *how* to see it, in reality afford the student little or no assistance beyond fixing the details of any process in his mind. For example, to read the account in the present work of the examination of a group of *Vorticellæ* or bell-animalcules, one would think that nothing could be easier than to follow the plain, clear directions therein laid down. But place even the expert student at the laboratory table, and direct him to display these animalcules as the manual teaches, and he will find that precept and practice in this respect are two very different things. We know of no work requiring more delicate manipulation than to display the details of structure, for example, of the "stalk" of the bell-animalcules; and we know, from practical experience, that in this, and in many other points, the work which reads so easy of performance may, in many cases, be very far ahead of the student's powers.

These remarks we write not to depreciate the aim of the present work, but to guard against the prevalent misconception that such works are to supply the place of the teacher. If any fault can be found with such manuals, it is that of making the details of practical work appear *too easy*, and of causing the student to regard *observation* as a process in which he is to be led, rather than, in the most useful sense of the process, to lead himself. As a text-book, along with the teacher's presence and direction, this work may be regarded as a valuable adjunct to the work of biological teaching. Professor Huxley has himself furnished the systematic descriptions of the various animals and plants noted, whilst Dr Martin has supplied the laboratory work in each case. We can hardly hope that, in ordinary university classes of natural history—save those specially adapted for purely scientific graduates—all the subjects noted in this volume can be practically studied; since they include the examination of both plant and animal forms in the true sense of biological work. But students of either branch of biology will find in the manual much that is well adapted for their careful study; and we need say nothing further, as indicative of our approval of the book as a literary production, than to state that the treatment of the subject-matter leaves nothing to be desired.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LV.—MEETING III.

Wednesday, 5th January.—Dr HALDANE, *late President, in the Chair, and afterwards* Dr GILLESPIE, *President.*

Dr Haldane, the retiring President, delivered the following valedictory address:—

GENTLEMEN,—My term of office as President of the Medico-Chirurgical Society having expired, it only remains for me to follow the example of my predecessors, and to address to you a few observations before leaving the chair to which you did me the honour of electing me two years ago. My immediate predecessor, Dr Handyside, in his admirable valedictory address, laid before us a full account of the origin and progress of our Society, and at the cost of great labour furnished us with what is in reality a most valuable index to its proceedings. My object is a much more humble one, being merely to indicate as briefly as possible, the nature and amount of work which has been gone through by the Society during the past two years. I shall arrange my observations under the different heads under which the medical sciences are generally grouped.

To begin with the Practice of Medicine: we have been indebted to *Dr Begbie* for two excellent papers, characterized, like all Dr Begbie's communications, by a full historical knowledge of his subject, accurate observation, and philosophical reflection. The first paper was entitled, "Albuminuria in cases of Vascular Bronchocele and Exophthalmos," and pointed out, what had not been before noticed, that this association, though not a constant, is a frequent one. The albuminuria, however, is not a permanent condition, but only shows itself after meals, particularly after breakfast, and seems to be altogether independent of any organic disease of the kidney.

Dr Begbie's second communication was on Hæmatinuria, and included a description of two cases of this singular affection: the pathology of the disease is still obscure, though Dr Begbie thinks it probable that it is dependent on a peculiar impression on the nervous system, as well as on a morbid condition of the blood. Warmth and quinine seem to be the most valuable curative agents.

Dr George Balfour, who had previously favoured the Society with a communication on the subject, gave the results of seven cases of Diabetes Mellitus, treated by a diet of skim-milk and animal food, accompanied by full doses of lactic acid. All the patients improved much under the treatment, and in one a cure seems to have been effected.

Dr T. J. Maclagan read an interesting paper on the Pathology of the Contracted Granular Kidney. Dr Maclagan agreed with Dr George Johnson in considering the elementary lesion to be a loss of the gland-cells of the convoluted tubules of the kidney, but differed from him in regard to the explanation of the hypertrophied condition of the coats of the small arteries. The paper was a suggestive one, and though the pathological conclusions are by no means to be regarded as established, was well calculated to elicit discussion.

Dr Stephenson, since appointed Professor of Midwifery at Aberdeen, laid before the Society an interesting communication on Eczematous Eruptions and Eczematous Asthma in Children. Dr Stephenson objected to the generally received classification of these eruptions, and narrated cases to show that asthma is not unfrequently associated with eczema.

Dr James Dunsmure communicated a singular case of a little boy five years of age, in whom any touch on the head produced a temporary loss of voluntary power, and in which the internal use of belladonna was followed by some improvement.

Among other communications connected with the practice of medicine may be mentioned *Dr Lawrence's* case of Intracranial Tumour; *Dr A. C. Campbell's* case of Cystic Tumour of the Kidney; *Dr Graham Steell's* case of Abscess of the Liver; *Dr Robert Pringle's* observations on Asiatic Cholera; and *Dr Cuthbert's* case of Obstinate Constipation in a boy six years of age.

As usual, the surgical members of the Society have displayed great activity, and we have had to thank them for many important communications.

Professor Spence laid before the Society the particulars of a case of Recurrent Fibrous Tumour. The case was one of extreme interest, the subject of it having been under observation for the long period of forty years. The tumour originally made its appearance in the left lumbar region, and was removed in 1832; it recurred three times, and was as often removed. On each occasion the growth presented the appearance of an ordinary fibrous tumour. Eleven years after the last operation, the patient again applied on account of a tumour of the right mamma, which was removed, but returned about two years afterwards, when a second operation was performed. About a year afterwards the patient died of pericarditis and broncho-pneumonia. Professor Spence, in commenting upon this case, observed that, though the growths presented some suspicious features, he was not disposed to characterize them as malignant, chiefly because during the whole period of forty years the patient's general health had remained unaffected, and because there were no indications of that cachexia which is met with in connexion with malignant growths. In the discussion which followed, Dr Matthews Duncan expressed his entire disapproval of the term "malignant cachexia." He considered there was no difference between it and some other forms of depraved health.

Professor Lister laid before the Society an interesting case of Rodent Ulcer, where, from the locality involved, a modification of the antiseptic dressing became necessary: instead of carbolic acid, boracic acid had been employed with a most favourable result.

Dr Heron Watson, in addition to exhibiting many pathological specimens, described to the Society a new operation for Excision of the Anterior Tarsus and Base of the Metatarsus, applicable to cases in which the disease is not so extensive as to justify amputation of the foot. He had performed the operation six times, and the results had been most satisfactory. On a subsequent occasion Dr Watson exhibited a patient in illustration of the amount and kind of consolidation which occurs after this operation.

Mr Annandale contributed a valuable paper, entitled "Antiseptic Incisions, an Aid to Surgical Diagnosis," in which he pointed out that in obscure cases of suppuration, in morbid growths of a doubtful nature, in diseases of joints, and in obscure cases of injury, much information might be derived from antiseptic incisions, while there was no danger of any bad consequences ensuing. Mr Annandale also read a paper on the Treatment of certain cases of Stricture of the Urethra by a combination of Internal and External Divisions.

Mr Joseph Bell communicated the particulars of a case of Retention of Urine of great urgency and difficulty, where he had employed Dieulafoy's aspirator to empty the bladder. Owing to the condition of the urethra it was impossible to pass an instrument, and from the enfeebled condition of the patient it seemed imprudent to perform perineal section. The operation with the aspirator was perfectly successful; the urethra was subsequently dilated, so that when the patient left the hospital, a No. 12 bougie could be passed. Mr Bell also read a paper entitled "Case of Operation for Intussusception in a Child." It was the opinion of the author, as well as of the surgeons who took part in the discussion, that when other means had failed an operation was justifiable. Mr Bell also communicated an interesting case of malignant pustule. The patient, a strongly made, previously healthy man, appeared to have contracted the disease from diseased cattle which he had been tending, and died on the fifth day after the first local symptoms of the disease. At the last meeting of the Society, Mr Bell read a paper on Vesico-Vaginal Fistula, in which he described three cases of a very aggravated character, on which he had operated with highly satisfactory results.

Dr John Duncan communicated an interesting note on the Subcutaneous Injection of Mercury. The author found that, under this treatment, secondary syphilitic symptoms disappeared more rapidly than when the drug was administered by the mouth, and that only a very small quantity of the mineral required to be employed. The preparation employed was the bichloride or bicyanide of mercury, $\frac{1}{16}$th of a grain in solution being injected at intervals of from one to three days. The physiological action of the medicine could generally be obtained after from $\frac{3}{4}$ of a grain to 1 grain had been administered. Dr H. Watson, in the discussion which followed, expressed his firm conviction that mercury is never required in the treatment of syphilis. He allowed, however, that, in so-called tertiary symptoms following the administration of mercury, mercurials were sometimes required to overturn the evil results of their former administration. In these circumstances, he believed that injection of the bicyanide of mercury as recommended by Dr Duncan might advantageously take the place of the use of mercury by the mouth. Dr Duncan also read a paper on the Treatment of Nævus, in which, from a consideration of the natural history of the disease, he was led to the selection of appropriate treatment in particular cases.

Mr Chiene communicated an important paper, containing an account of the dissection of a recent case of Colles's Fracture of the Radius, with special reference to the pathology and treatment of the lesion. The reading of the paper was followed by an animated discussion. Mr Chiene also communicated a case of Absorption of an Exostosis; and read a paper entitled Bloodless Surgery, in which he demonstrated, apparently to the satisfaction of the Society, that Esmarch's elastic bandage and elastic ligature are not improvements in surgery.

Dr Alexander Miller read a paper on "Two Cases of Disease of the Knee-Joint treated by Sayre's Splint," in which he described a modification of his own, by which the disadvantages of the splint in question were got rid of.

Dr Blair Cunynghame communicated a remarkable case of Mechanical Obstruction of the Flow of Urine for eleven days, and in which no symptoms of uræmia manifested themselves. In the opinion of the author, the condition had probably been produced by the obstruction of the single ureter of a horse-shoe kidney, or of the ureter of the left kidney (the right kidney being incapable of secretion) by a uric acid calculus.

Dr Macdougall read a paper on some cases illustrative of the Principles of Conservative Surgery, and was warmly congratulated by the Society on the success which had attended his treatment.

In the department of Ophthalmic Surgery, *Dr Argyll Robertson* read a paper on the Operation of Repositio Ciliorum in cases of Trichiasis. In suitably selected cases, Dr Robertson had found the operation very successful.

In connexion with diseases of the Female Generative Organs, *Dr Foulis* read two important communications with reference to the Morbid Conditions of the Ovary, and showed of what importance, for diagnostic purposes, was a microscopic examination of the fluid withdrawn by tapping from the cysts.

The Society has been indebted to several of its members for contributions connected with Medical Jurisprudence.

Dr Littlejohn communicated two cases of Accidental Suffocation. One of these occurred under highly suspicious circumstances, and but for the very careful and accurate investigation of himself and the other medical witness, Mr Joseph Bell, might have placed an innocent man in a very unpleasant, and even perilous, position.

Mr Joseph Bell communicated some interesting surgical cases bearing on Medical Jurisprudence.

Dr Heron Watson made a communication with reference to an investigation in which he had recently taken part, in connexion with which a man had been tried for the murder of his wife. The woman was found dead in bed, with an incised wound penetrating the chest, her husband having been the only person present in the house at the time. Dr Watson, from a carefully devised experiment, came to the conclusion that the wound might have been inflicted accidentally. This case, as members of the Society are no doubt aware, has led to a prolonged controversy; but as that controversy did not take place within the walls of the Society, it cannot here be farther alluded to.

Dr Taylor communicated an interesting case of Poisoning by Coal-Gas, where a gentleman, unaccustomed to the use of gas, lost his life by having blown out the gas-flame in his bedroom.

Dr Cuthbert read a case where a child, two years and a half old, nearly lost her life from having swallowed two drachms of Bow's liniment, and where the employment of electricity appeared to have had a markedly beneficial effect.

Finally, under this head, or rather that of Medical Statistics, *Dr Gillespie* read a Report on the Causes of Death among the Assured in the Scottish Union Insurance Company.

In the department of Physiology, *Dr M'Kendrick* read an important paper, entitled "Comparative Observations on the Physiological Action of Chloral and Bromal Hydrates and Iodoform." The general results arrived at were, that bromal hydrate is more active physiologically than chloral hydrate; that bromal hydrate acts less vigorously than chloral hydrate on the cerebral hemispheres, but more powerfully on the ganglia at the base of the brain and on the spinal cord, and consequently generally produces convulsions; that chloral hydrate does not usually produce great contraction of the pupil, but that bromal hydrate always does.

Dr Cappie read an elaborate paper on the Relations of the Cranial Contents to the Pressure of the Atmosphere; and *Mr Gilruth* read a paper on the Physiological Effects of Section of the Spinal Cord in the Lower Animals.

Papers connected with the subject of Materia Medica were not numerous, the most important being two communications by *Dr Craig* on Jaborandi, and a paper by the same gentleman on the External Uses of the Hydrate of Chloral.

Such, Gentlemen, is a very brief record of the principal papers which have been laid before the Society during the last two years; and I think that, whether as regards the number, the variety, or the importance of the subjects treated of, the Society has no reason to be ashamed of the activity of its members. I have passed over in silence a very large number of minor communications connected with the presentation of patients, and the exhibition of morbid specimens, which, when kept within due limits, add largely to the interest of our proceedings.

It now only remains for me to thank you again for the honour you conferred upon me by electing me your President, and, in retiring from the chair, to express my great satisfaction that I shall be succeeded by one of my oldest friends, one who has already done much for the Society, Dr Gillespie.

Dr Gillespie returned thanks for the honour conferred on him. He had accepted the office with some diffidence, as recently he had not attended the various meetings with much regularity. Like many of the older members, he found difficulty, as he got up in years, in turning out at night. One compensation, however, would be the listening to papers such as those which Dr Haldane had so gracefully alluded to. He only wished, in conclusion, to move a vote of thanks to Dr Haldane for his admirable address.

The following Pathological Specimens were shown—

I. *Mr Annandale* showed a CAST OF A RARE INSTANCE OF OSSEOUS TUMOUR ON ANTERIOR ASPECT OF KNEE, as well as the tumour itself after removal. The patient was a middle-aged man, with a tumour over the knee, hard and fixed, especially when the lower limb was flexed, so that it resembled an osseous tumour attached to the head of the tibia. When, however, the knee was bent there was a little movement. He, therefore, with antiseptic precautions, removed it by a free incision. It was then found to be under the ligamentum patellæ, so that its fixity was accounted for. After its removal, the whole synovial membrane on the anterior aspect of the joint was seen to be exposed; but yet, owing to the antiseptic precautions, no unfavourable symptoms followed, and the patient had now a perfectly movable joint. The tumour itself was partly osseous and partly cartilaginous. He had never seen a precisely similar case.

II. *Dr A. G. Miller* showed a TUMOUR OF THE MAMMA he had removed. The peculiarity was, that in its upper part there was a cyst, while the lower part was scirrhous. The presence of the cyst was not accurately made out before operation, as the cyst was embedded in the substance of the mamma, and was supposed to be part of the scirrhous disease, which latter was evident from retraction of the nipple and other symptoms. The wound had healed kindly and soundly.

III. *Mr Chiene* showed ELEVEN CALCULI, removed from under the prepuce of a native of India by Dr Warburton. The patient was a man æt. 27, with phymosis following a gonorrhœa, contracted three or four years previously. The prepuce had been slit up, and the stones shown removed. Their number and size were rare; and he had therefore brought them under the notice of the members.

IV. *Mr Chiene* next showed a CHAIN-SAW, with a chain at each end, which could be wound round the fingers, and a firm grasp thus given. In this way, the use of handles, always a difficulty in the proper management of chain-saws, was avoided.

V. *Dr T. G. Balfour* showed a remarkable SPIRAL HORNY TOE-NAIL. One peculiarity was that it was hereditary, as the patient's mother and two aunts were affected in the same way. A cousin also on the mother's side was similarly troubled with such enlargements. The specimen had first appeared when the patient was thirteen years of age, and had not been cut for five or six years.

VI. *Dr Argyll Robertson* then read a paper on TREPHINING THE SCLEROTIC IN GLAUCOMA.[1]

He stated that ophthalmic surgeons at the present day generally agreed in attributing the symptoms present in most cases of glaucoma to an increase in the intra-ocular pressure. All measures therefore that have been advocated as serviceable in glaucoma have for their object a reduction of the increased intra-ocular tension. The chief of these were—paracentesis of the cornea, division of the ciliary muscle or intra-ocular myotomy, and iridectomy.

Paracentesis of the cornea, while undoubtedly capable of alleviating the symptoms, produced only temporary benefit. Division of the ciliary muscle, or intra-ocular myotomy, had fallen much into disuse, partly, he believed, owing to the temporary character of the benefit following it, and partly from the liability of all wounds in the ciliary region to be followed by chronic insidious inflammation leading to loss of vision, and even to sympathetic affection of the other eye. Iridectomy was the prevailing remedy for all glaucomatous affections. While this operation, he willingly admitted, effected brilliant results in many cases, reducing permanently the intra-ocular pressure, and saving sight, there were exceptional cases, in which the removal of a portion of iris could not be satisfactorily effected, and others in which the operation failed to afford relief. Dr Robertson was thus led to devise an operation which might prove serviceable in such cases. The operation consisted in trephining the sclerotic, thus making a circular aperture into the chamber of the vitreous humour about $\frac{1}{12}$ of an inch in diameter. The escape of some of the contents of the eye at once served to reduce the tension, while the author believed that this effect was probably permanent, as the circular opening in the sclerotic must be filled up by new tissue having less resisting power than the original membrane, and would thus readily yield to pressure from within, acting the part of a safety-valve, should at any future time the contents of the vitreous chamber be increased in amount.

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[1] The paper will appear in full in an early number of the Royal Ophthalmic Hospital Reports.

Four cases had been subjected to the operation, and in all the trephining had been effected by means of Mr Bowman's cornea trephine, but this instrument did not penetrate the tough sclerotic so readily as might be desired, and did not give a firm hold to the fingers in the rotatory movements necessary for trephining; so Dr Robertson had got an instrument constructed to obviate these disadvantages, which he exhibited to the Society. In all the cases, the aperture was made through the upper part of the sclerotic, as the eye during the trephining could be pressed against the unyielding inferior orbital plate; the point chosen was about two lines from the margin of the cornea, corresponding to the junction of the ciliary processes with the choroid. The four cases were given in detail: the *first* was that of a girl affected with extensive staphylomatous protrusion of the sclerotic, to such a degree as to interfere with the movements of the upper lid. The tension of the eye was very markedly increased. The cornea was the seat of a dense opacity, and the pupillary margin of the iris adherent all round to the capsule of the lens. There was mere perception of light in the eye. After the operation, the staphyloma completely subsided, and the general curvature of the eye became re-established. The tension was much diminished immediately after the operation, and remained permanently a little less than normal, and the patient's vision slightly improved. After about three months, the patient returned with the eye in a slightly irritable state, and tender to the touch. As the eye was practically useless, and the other (which was also diseased) might become sympathetically affected, enucleation was recommended, and, with the patient's concurrence, effected.

The *second* case was that of a man, whose left eye, when he came for advice, presented all the characters of acute glaucoma. Vision had been completely lost for about three weeks. An iridectomy was at once performed, but this only produced temporary relief—the wound did not heal, and the tension remained increased, while in a week the pain became again so severe as to necessitate further measures. Having noticed great diminution in tension follow the trephining practised in the previous case, Dr Robertson deemed it advisable to test the efficacy of the operation in this one. The result was that the wound in the cornea healed at once; the tension was much diminished, and the pain removed; but in a short time the tension became again increased, and the pain returned; the symptoms yielding temporarily to puncturing of the coats of the eye at the trephined spot (which bulged forwards). This constant recurrence of pain and increased tension, along with rapidly progressing opacity of the lens, led to the suspicion of the presence of a tumour in the interior of the eye. Eventually, the eye was enucleated, and a growth found occupying about a half of the chamber of the vitreous humour. [The enucleated eye with tumour was exhibited to the Society.]

The *third* patient was a man 57 years of age, whose left eye was affected with advanced glaucoma, attended with complete loss of vision, of more than a year's duration. For about two months before applying for advice he had suffered periodically from severe pain in the left eye, extending to the temporal and frontal regions, frequently preventing sleep. By the operation the patient's sufferings were at once relieved. When last seen (three months later), he had had no return of pain; the tension of the eye remained diminished, and the pupil contracted when light was admitted to the neighbouring eye. On ophthalmoscopic examination, only a dull-gray reflection was obtained from the interior, which was produced by a grayish-coloured membrane that stretched from the point of aperture in the sclerotic across the chamber of the vitreous humour. Dr Robertson attributed the presence of this membrane to blood-extravasation into the vitreous chamber at the time of the operation.

The *fourth* case was that of a girl 21 years of age, affected with chronic glaucoma of both eyes. The disease had been of six months' duration in the left eye, while the right had been similarly affected for six weeks. The tension in both eyes was decidedly increased, and the sight considerably impaired in the left. Vision in her right was almost normal. Dr Robertson trephined the sclerotic in the left, the worse eye, and performed an iridectomy on the other. She made a rapid recovery from both operations. In both eyes the tension was reduced to the normal degree, and when last examined, three months later, it had remained so, while the vision (after adapting suitable lenses to correct acquired astigmatism in the eye in which iridectomy was performed, and defective power of accommodation in the other) was in each almost exactly the same as prior to the operations, while he felt confident that this intractable disease had been checked. [This patient was exhibited to the Society.]

In commenting on these cases, Dr Robertson said that the result in the *first* case exceeded his expectations, as, in addition to the subsidence of the staphyloma, the tension, which was previously excessive, remained permanently diminished, and vision was slightly improved. The sympathetic irritation of the other eye, which eventually induced him to enucleate the trephined eye, he attributed to the diseased state of the eye, and not to the trephining.

In the *second* case, where the increased tension was due to an intra-ocular growth, and where iridectomy failed to afford relief, the trephining was of decided benefit, ridding the patient of pain and diminishing the intra-ocular pressure, which the author considered irresistible proof of the efficacy of the operation.

In the *third* case, the operation effected all that was desired—causing diminished tension and relieving pain.

In the *fourth* case, he considered that the trephining in the one eye had been relatively quite as successful in its results as the iridectomy in the other.

As the result of his experience, Dr Robertson expressed his conviction that, in this operation, we possess an effectual means of reducing increased intra-ocular tension, and that in most cases it will be found effectual in preventing a return of increased tension, or at any rate in warding off the evil consequences of that condition.

The President felt sure that the Society was much indebted to Dr Robertson for his admirable contribution to the treatment of glaucoma. He thought that Dr Robertson deserved credit for his perseverance, as the results in his first two cases were not so good. This had happened in the second case, simply because no treatment would have caused a good result. But as he felt he was theoretically right, he had continued to use it, with the satisfactory results given in the third and fourth cases.

Mr Annandale, although not an oculist, yet, as a surgeon, could judge of the value of the paper, and congratulated Dr Robertson on it. He was well aware that the operative treatment of glaucoma had been disputed; but as its object was to overcome tension, he could therefore judge of the value of Dr Robertson's method. One thing that struck him was the little irritation following the operation. He wished to suggest that antiseptic measures and drainage, by means of a fine drainage-tube, might be employed. He only further wished that he knew more on the subject: but he thought he knew enough to recognise the value of the paper.

Mr Bell wished to add to what the President and Mr Annandale had so well said. Dr Robertson's cases proved that the operation did no harm. The first two cases proved nothing against the operation, because the first was a bad case; and in the second, iridectomy had also failed; so that the pain and tension were caused by something else, viz., the tumour in the posterior part of the eye. Its detection was indeed a good piece of diagnosis. The third case was a good one, as nature pointed out by the posterior staphylomata that the tension was in the posterior part of the eye. He agreed with Dr Robertson as to the risks of extravasation. He thought the operation would be best in young patients, and those with healthy arteries. If atheroma were present, the risk of extravasation would be greater. The patient in whose case it had happened was rather advanced in years, as he was 57 years of age. He thought that in such cases there should be gradual relief of the tension by some method. We know that if the distended urinary bladder of an elderly person be completely relieved, there is risk of death from hæmorrhage; not from any ulcer, but from the smaller vessels of the bladder being suddenly relieved of pressure. He therefore thought that in trephining in glaucoma, it might be so done that the fluid gradually drained off before the trephine was removed. The result in the fourth case was very good, as the trephined eye was the worse. One advantage of his operation Dr Robertson had not alluded to, viz., the æsthetic one, that there was less deformity than in iridectomy. In the third case, indeed, there was a little adhesion of the iris; but this was not a necessary consequence. He finally wished to congratulate Dr Robertson, as he had often done before, on his admirable paper.

Dr Argyll Robertson, in reply, thanked the members for their favourable criticisms. As to the suggestion about antiseptics, he had not yet used them, because their most active agent was so irritating. He might, however, use milder ones. But the success with old measures was so good, that he did not feel inclined to dabble in new ones. Possibly, the fact of the eye being covered with the eyelid made the operations on it practically subcutaneous. He was much indebted to Mr Bell for his interesting remarks on the cases. The gradual drainage Mr Bell had advocated, had indeed also occurred to himself. He thought it might be effected by trephining down to the choroid, and then puncturing that membrane so as to allow the fluid gradually to trickle through. Rupture of vessels was due to sudden diminution of tension.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXV.—MEETING III.

Wednesday, 8*th Dec.* 1875.—Dr JAMES YOUNG, *Vice-President, in the Chair.*

AFTER the Fellows, and a number of visitors who had been invited to be present at the meeting, had partaken of tea, the minutes of last meeting were read and approved.

Dr Young introduced Professor Simpson as President; and read letters of apology from Professors Laycock, Gairdner, and P. H. Simpson, Glasgow; Dr Fleming, Glasgow; Dr Morehead, Mr Brown, Dr Orphoot, Mr Craig, and Mr Smith, Colinsburgh, regretting that they were unable to be present.

The *President* then delivered an address upon MENSTRUATION.

Dr Matthews Duncan proposed a cordial vote of thanks to Dr Simpson, which was seconded by *Dr Moir*, and carried by acclamation.

Dr Simpson replied, and thanked the strangers who were present for coming.

Dr Littlejohn expressed the pleasure it had given him to be present at the meeting.

Part Fourth.

PERISCOPE.

PROSTITUTION.—We give an abstract of an able paper on this subject by a writer who signs himself "Democritus Contemporalis," *New Orleans Medical and Surgical Journal*, Nov. 1875:—

The origin of prostitution is probably coeval with society, and one form of venereal disease, at least, resulting from it was made the subject of Mosaic enactments.

At Rome prostitutes were registered from time immemorial. At Athens there was no disgrace in visiting the public brothels, or in keeping a mistress. This last, in fact, was the social standing of the most celebrated women whose names are handed down to us.

Our Saviour's conduct in the case of the woman taken in adultery, and in that of Mary Magdalene, shows that He did not wish to enforce the bloody penalty of Moses for adultery, nor shut the door of repentance on the fallen, as in the Roman law. It is becoming in Christians, while considering this question, to bear these lessons in mind.

Some of the early fathers looked upon prostitution with some allowance. St Augustine said, "Suppress prostitution, and capricious lusts will overthrow society."

In France fruitless efforts were made to suppress prostitution by Louis IX. in 1254. In two years the plan was abandoned, as it resulted in transferring the impurity from the brothel to the family circle. There has been no direct legislation on prostitution in France since the Revolution, but the Government has assumed control without warrant, and the police regulations have acquired force by custom. The French plan of police supervision has removed some of the most repulsive features of prostitution, by compelling abandoned women to observe a decent deportment in public.

M. Lecour, in *Archives Générales*, 1867, asserts that prostitution is now undergoing a transformation in Paris. There is a decrease of registered prostitutes and tolerated houses, and a corresponding increase of uninspected and clandestine prostitutes. At Hamburg, also, punitive measures have proved unsuccessful in suppressing prostitution.

In Berlin the closure of the brothels was followed by a large increase of private prostitution, and a greater prevalence of venereal disease. In 1851, the irrepressible and increasing evils of illicit prostitution, which had defiled the domestic circle with moral and physical corruption, led to the restoration of the toleration system.

Dr G. Richelot, in 1857, states, that on the closure of the brothels in Berlin, the number of clandestine prostitutes known to the police increased in one year from 700 to 1250, and in the four years preceding 1850, to 8000. Syphilis increased, and invaded the best families; unnatural offences became frequent, and bastardy common. On the re-establishment of tolerated brothels these evils diminished, and illicit prostitution declined.

At Stockholm there are no regular brothels, prostitution being prohibited, but syphilis is exceedingly common; and the jurist Angélot asserts that every house of entertainment is a brothel, and every female servant unchaste. The ratio of illegitimate births to legitimate was 1 to 2·38, and in the other Swedish towns 1 to 6·40, in the year 1839.

In New York, Dr Sanger estimates the average duration of life among prostitutes at four years. At Paris it considerably exceeds this. Dr Sanger proposes to place prostitution and prostitutes under the supervision of a medical bureau in the police department. For this purpose there would be needed—

(1.) Free police visitation. By thus making the faces of prostitutes familiar to detectives and the police generally, they could easily be kept from public assemblies. Let prostitution be isolated from all other means of indulgence, and not only will it be deprived of the material support of its allies, but its innate depravity will be clearly revealed, and it will be branded with the infamy it deserves. Police supervision would prevent the solicitation of passengers by public women on the streets and at their doors and windows,—practices now so common and so shocking to decency.

(2.) The sanitary condition of the dens of infamy would be improved.

(3.) Reformatory measures could be more easily addressed to prostitutes. Besides, it often happens that young girls are enticed to secret dens and detained, forcibly or otherwise, until their ruin is complete.

(4.) A special tax to be levied on property used for purposes of prostitution, and a capitation tax on public women.

(5.) But the great end proposed is the prevention of venereal diseases.

The writer offers the following propositions as a basis for regulations:—

(*a.*) The registration of all known prostitutes.

(*b.*) Medical inspection of registered prostitutes at short intervals; the removal to hospital, and detention till cured, of the diseased.

(*c.*) Let it be made a penal offence, and a ground of action for private damages, for any person to communicate venereal disease to another.

With regard to the third proposition, he says:—When the terrible results of syphilis are considered, together with the fact that, in its primary form, in a great proportion of cases, it is well known to its subject, and communicated either maliciously or heedlessly, it is time that it were recognised as a penal offence to give it to another person. While this would bear equally upon prostitutes and their paramours, a prosecution for private damages would be especially operative against the latter, who share too little the odium of an evil in which they are participants.

The disinclination of the party aggrieved to prosecute would be the most serious drawback; but cases would now and then be brought into court, so as to lead to some terror of the law.

Prostitutes, when found diseased, if possible, should be made to designate the source of infection; and if the parties accused should be found similarly diseased, and the intercourse fairly proved, the case might be brought home to conviction. Also, male venereal patients in the public hospitals should be made to account for their disease, which would frequently tend to bring clandestine prostitutes under police supervision.

There is little danger that it would be used for purposes of personal spite or extortion, for if the accused party were free from disease, the result would be an acquittal, and a probable countercharge of malicious prosecution.

G. HOMOLLE. *Lupus of the Mouth and Pharynx.* Paris, 1875.—Lupus of the mucous membrane of the mouth and pharynx may occur either as a complication of lupus of the skin or as a primary affection of the mucous membrane, the integument being quite free. The latter cases are the most rare, and their diagnosis is attended with more difficulty than that of the former. In the first class of cases the disease of the mucous membrane presents the following types:—1. A livid erythema, in which the mucous membrane is strikingly dry and shining. 2. Granulations. 3. Hypertrophic, in which there may be mulberry vegetations, and there may be also destruction of the gums. 4. An atrophic type (interstitial atrophy); this is rare, and is generally represented by very superficial ulcerations. 5. Ulceration; this may be of the nature of a progressive erosion, or it may be deep, but with generally an atonic and torpid character. 6. Perforation. 7. The exceedingly rare transformation into epithelioma.

The seat of the affection has an important effect on the appearance. Thus the lips and velum are generally of a livid red, whilst the soft palate is mostly ulcerated and liable to perforation. The tonsils are usually free.

The cicatrices which remain are, according to the author's views, not characteristic. They are depressed and pale, or of a brown colour. Also, a radiated form of cicatrix, which is generally believed to be characteristic of syphilis, occurs in the best marked cases of lupus; more rarely union between the velum and pharynx.

A certain indolence of character is to be noted as a functional symptom; even when the destruction extends deeply, it may be unnoticed and without a symptom. There is also mostly diminished sensibility of the mucous membrane.

The primary lupus presents the same lesions as that propagated from the skin. There are here two forms to be clinically distinguished, the primitive lupus and the primitive ulcerating scrophulide. By the first the parts are undermined, nibbled, and atrophied, by the latter they are destroyed, disintegrated, and perforated. In primary lupus the appearance of a single tubercle is exceedingly rare, much more frequently the tubercles are conglomerate, frequently hypertrophic in type. The soft palate is mostly attacked first, but the velum and pharynx are soon affected, and there is destruction of tissue and adhesion. The ulcerating syphilides begin at first with suppuration, and lead very early to loss of tissue. They produce on the velum either suppuration on the free edge, perforating ulcers, or they divide it into separate portions. On the posterior wall of the pharynx they form sharply-margined ulcers of an atonic character. The epiglottis is very seldom primarily affected.

Symptomatically these primary affections of the mucous membrane have not such an indolent course as the secondary lupus. There is difficulty of swallowing. The infiltrated parts show early symptoms of paralysis, and the sensibility is decreased. If there is now added mutilation and perforation, the solid food and still more the fluids regurgitate, the voice is coarse, in perforation it is nasal, and when the larynx is seriously affected there may be complete aphonia.

As complications are swelling of the glands, keratitis, conjunctivitis, and albuminuria, but these are not constant. Œdema glottidis is rarely induced. In the general constitutional condition there are four clinical types to be distinguished. Sometimes the patients seem quite healthy and strong, but this is rarely the case in the ulcerating forms. Others look well, but show remains of previous scrophulous affections, such as opacities of the cornea, and cicatrized glandular swellings. In a third group the scrophulous condition is active. There are large indolent glandular swellings, cicatrices, red eyelids, etc. Fourthly, some young patients exhibit the symptoms of hereditary syphilis,—have the "syphilitic look" of English observers,—a square nose, on the whole faulty bodily development, serrated teeth, etc. Here there are mostly perforations and serpiginous ulceration.

The individuals affected are mostly between 10 and 20 years old; sometimes under 10, rarely over 40.

The author, in express opposition to the schools of Berlin and Vienna, regards lupus as a scrophulous disease *par excellence*, and designates it, therefore, especially in its ulcerating forms, as a *scrophulide*. Whether lupus may be considered as a form of hereditary syphilis, he leaves undecided.

The diagnosis of all forms of primary lupus of the mucous membrane is very difficult. The idea of syphilis is at once suggested, and only a consideration of the general state of the patient, and the history, afford grounds for a decision. The ulcerating cases are the most difficult, whilst the others, even progressing erosions, can be more easily recognised. The result of treatment does not much help the diagnosis, as antisyphilitic remedies, such as iodine, have a beneficial effect in lupus.

The prognosis is always doubtful—not as regards life, but in regard to a complete restoration to health. A cure is never certain, as it is liable to be interrupted by frequent relapses.

The treatment is hygienic, country or seaside air. Inwardly, cod-liver oil and iodine are most beneficial, the latter especially after a previous long course of the oil. Mineral waters containing bromine and iodine, as Kreuznach, Nanheim, Sulies, etc., are to be recommended. Outwardly, painting with undiluted tincture of iodine, chromic acid, chloride of zinc, and nitrate of silver, should be employed; and when the pain is severe, iodoform in glycerine; and, finally, washing with chlorate of potash and alum.—*Translated from a Summary by O. Simon in the Centralblatt,* No. 55, 1875.

A VACATION HOMILY.—We cull the following from the September number of the *Boston Medical and Surgical Journal:*—

Messrs Editors,—"Americans know nothing of the art of taking rest," is the incisive remark of some magazine writer who is an acute and truthful observer of the habits of our people. He might have added that Americans are apparently ignorant of the necessity of rest, and of the way in which it is to be found. What is the summer rest of those of our people who recognise their physical needs? Some—the few, of course—court rest in entire change of scene and daily habit by steaming to Europe, the delightful do-nothing of the eight or ten days at sea relaxing overstrained nerves, and bestowing a benefit which is intensified, let us earnestly hope, by a thorough revolution in the stomach during the first twenty-four hours, the liver being meanwhile jerked and shaken into friendly sympathy and a future reinvigorated action.

This feature of the sea-trip would hardly be voted delightful. No voyager would choose the unsentimental sensations and deprivations of "the first day out," but who ever failed to find an appetite for every one of the five meals of the steamer, and the nine o'clock P.M. Welsh rare-bit or sandwich besides, after having spent twenty-four or forty-eight hours in Neptune's mangle? In the exhilarating days which follow, should not this voracious being bless the rough handling of the sea-god! What bitters or tonic ever did so much for him? The sea-air, the lazy life, the freedom from care, and the hearty appetite, prepare our traveller for an active, enjoyable, healthful run from lake to mountain, from cathedral to village hamlet, from picture-gallery to sea-view. He lives in an unaccustomed world, thinks unaccustomed thoughts, and, better, sleeps dreamless sleep long unknown. In October he returns to ten new days of steamer life, and reaches home with youth half renewed, and a fund of vigour which will endure beyond belief.

This is a wise manner of reviving sapped forces, but it requires a long purse. Fortunately, there are other equally helpful means of recreation. Another small portion of our people is sensible enough, and plucky enough, to "sink the shop" out of sight, and become, as nearly as may be, children of nature, by following the better habits of the aborigines. There is probably no more healthful method of refreshing brain and body than that offered by camp life—life out of doors day and night; the meaty portion of the food of the camper-out being taken by himself from air, water, or woods; the day a succession of invigorating tramps, canoe-paddling, shooting, or fishing; the night a long, sweet, untroubled oblivion beneath the sky.

I believe the surest of prophylactics in incipient consumption, and the best of tonics in nervous affections and general debility, might be found in this simple advice—live an out-of-door life, and never sleep under a roof heavier than a tent-cloth. Such advice in our climate would of course, in certain months, require change of latitude, and perhaps a margin of pocket-money, but the law of effects would nevertheless hold good. The camper-out begins his rest by entering upon Indian habits. He returns to labour with the swarthy hands and face of the aborigine—with muscles as tough, appetite as sharp, and brain as clear as his. The store of vigour which one can pack away during a month or six weeks of camp-life is amazing. More amazing are the "staying qualities" of this toughened, hard-packed endurance. To the astonishment of the heretofore dyspeptic, headachy, weak-marrowed, lifeless plodder, the old gaps in life are bridged over, the stomach behaves itself charmingly, the brain acknowledges all drafts at sight, and the nervous mechanism runs without a click or a jar, perhaps even into the new summer months which call for a return to canvas walls and the big bath in sunlight and oxygen. These campers-out are sensible creatures. With a small investment of cash they win immense dividends in a commodity too invaluable to be mentioned in the same breath with gold. But these speculators at nature's stock-board are unfortunately too few. The average professional or business man thinks he cannot leave his duties even for one month out of the twelve. This must be an absurd error. A well-managed business can surely be left during the dull months, and in the temporary absence of its head, to the care of subordinates, who may rest in their turn; and professional men can always relieve each other in the absence of either. This is done in other countries. It is only in America that men work until they wear out, or rather break down. It is haste to become rich that renders riches so useless, for one never has time to enjoy them, and the physical smash-up comes long before a well-conducted organization would dream of sinking under the effects of fatigue.

Another class of business and professional rest-seekers are those who go out of town for a two days' stay, then return to business for an equal length of time, and thus oscillating between labour and refreshment, fail to enjoy either. During their absence they continually think of business, and so never free their minds from its claims, thus defeating all the restorative attempts of the nature they seek. This is another absurdity. In order to obtain rest the ordinary occupations of life must be absolutely dropped, and, if possible, forgotten.

Perhaps it is the many who in seeking rest take upon themselves all sorts of ridiculous hindrances, annoyances, and unhealthful conditions. These are they who crowd the fashionable resorts—who, leaving their comfortable homes behind them, with sweet cheerfulness stive themselves in seven-by-nine rooms; dress for breakfast, for dinner, for promenade, for evening; follow the fashionable routine; fear the sun, the wind, and the rain; eat questionable meals, bearing discomforts unnumbered for fashion's sake, and live in houses which are ill aired, poorly drained, typhoid breeders in many instances. For your fashionable landlord thinks more of economizing space, of filling stuffy apartments at disproportionate rates, than of pure air and well-built drains. There is neither economy nor refreshment, and but little pleasure, in such summer recreation. I know of a lady who passes her summers at a watering-place, and who refuses to receive her callers, actually depriving herself of what she confesses would give her great pleasure, because she finds it too much trouble to dress *twice* daily after her bath. The absurdity here lies in living under such rules as oblige one either to dress several times between rising and retiring, or to accept the dreadful alternative of being considered unfashionable. Fashion and restfulness are as incompatible as oil and fire.

But, undoubtedly, the great objection to fashionable summer resorts consists not in the mere failure to secure repose, or even healthful recreation, so much as in the effects of the neglect of hygiene in the arrangements of hotels and boarding-houses. Landlords care for nothing but gain, and their guests think of anything else than good water and suitable drainage. If typhoid be thus contracted, and the victims die, their friends thoughtlessly say (to quote a writer in *The Atlantic Monthly*), "In the wisdom of an inscrutable Providence, it has been found necessary to remove them from our midst. In this way we blandly impose upon Divine Providence the responsibility of our shortcomings. The victims of typhoid fever die, not by the act of God, but by the act of man. They are poisoned to death by infections that are due to man's ignorance or neglect." The subject of summer rest cannot be adequately treated within the limits of a brief letter. But this much can be shortly said: we all need rest. It need not be said that I refer especially to those who have earnest vocations in life. There are those who never do anything. Such could by no possibility find rest in anything but work. To the actual worker a full month of positive forgetfulness of the eleven months of duty is a necessity. Business, whether professional or mercantile, should be put under mental lock and key, and the key lost for at least four weeks of the fifty-two. Power to work becomes intensified through rest. Hence to take rest becomes a duty, and to none more than to the physician. He cannot properly do his work when depressed by physical and mental fatigue. He holds immense responsibilities in his care. An error which might be the result, not of carelessness, but of weariness, of benumbed faculties, could work him incalculable harm, fetter his future usefulness, stunt his professional growth; besides, and worst of all, perhaps taking a life. If any worker needs a clear brain, a light heart, a vigorous body, it is the physician; and yet I suppose few men are so unready to rest as are medical men. This is a palpable wrong, both to their patients and to themselves. To thoughtful minds this mere word should be enough.

Let the physician, then, take his gun, his boat, his fishing-rod, his novel, and hie himself away from work to rest. It is not necessary to live a tent-life, if he but take sensible rest otherwise. Let him avoid hotels and boarding-houses, and take his family (for let us hope he has one) to some clean, airy farm-house, where milk is plenty as water; where eggs, chickens, lambs, vegetables, and the summer fruit of the country are abundant and fresh. Whatever lameness may be evident in cookery may be easily set aright by a few hints from his wife.

As to dress, which is one of the immense bores of fashionable resorts, our rusticating physician will find great delight in the comfort of navy-blue flannel overshirts. Linen should be reserved for evening wear. During the day, whatever be the amusement, coats, vests, and linen should be discarded. Nine o'clock P.M. is a sensible retiring hour, and if the day have been properly spent, by this time one is ready for sleep. The physician—and this may apply to every class of reading men—may be followed, if he choose, by current professional literature and one daily newspaper; otherwise let him leave all books, except novels, behind. Recreation is much enhanced if the summer location be near a lake; for rowing, sailing, fishing, and swimming are not only great delights, but they also serve to purify the life-current, and harden the muscles. One month of such life is like a dip into the fountain of youth. One great improvement may be suggested: make the four weeks six. Rest of this nature should be governed by one general rule, namely, between morning and evening live out of doors. How spend the time? Walking, driving, fishing, hunting, berrying, picnicking, sailing, rowing, swimming. To such enjoyments the days go only too swiftly, and night brings sleep worth a king's ransom.

If our physician live in the country he may secure similar pleasures with equal simplicity and variety at some quiet seaside village.

Such is the simple, economical, healthful rest which we may all find. It may be varied in a thousand ways, according to one's disposition and inclination. If any candid mind do not recognise the great desirableness and healthfulness of a summer passed in this manner, as compared with life at the stupid, mismanaged, too often unhealthful resorts where one is trammelled and pent up in all ways by the demands of dress and etiquette, and exposed to sickness by the cupidity and ignorance of landlords, such a mind must have been warped by the very life we should all flee when we seek rest.—X.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal.*]

The Physiological Action of Arsenic.—According to Kossel and Gähtgens, the action of arsenic may be regarded as analogous in several respects to that of phosphorus. Poisoning by the latter agent is characterized by the deposition of fat in the various organs, an effect which is also produced by arsenic and antimony. From the experiments performed it would appear that the nitrogenous constituents of the cells undergo disintegration, and that the nitrogen is eliminated in the form of urea.—*Centralblatt für die Med. Wiss.*, No. 32, 1875.

The Action of Alcohol.—In the *Giornale Veneto delle Scienze Mediche* for March 1875, is a summary of the observations of Drs Felice Lussana and Pietro Albertoni on the action of alcohol. These authors point out how, among the phenomena of acute alcoholism, disturbance of the intellect prevails in man, while in dogs, and still more in birds, the phenomena of muscular disturbance are most prominent. This is compared with the anatomical fact, that in man the development of the nervous centres of intellect surpasses that of the motor centres, while the reverse is the case with dogs and birds. The authors conclude that the action of alcohol on the nervous system is in proportion to the relative development of the nervous centres. And this would appear to be in conformity with the general principle, that the action of the drug on various animals is in direct relation to the greater or less development of the organs on which it specially exerts its influence. It may hence be argued that a substance will act in a smaller dose in proportion to the perfection of the organ on which it exerts its elective action.

With regard to the absorption of alcohol, it appears from the authors' experiments that this is subordinate to its degree of concentration. If this be great, alcohol exerts a strong local irritant action, but a feeble general action; inasmuch as the part congested, deprived of its normal conditions, has its capability for ready and complete absorption impaired. The authors prove that, besides being absorbed by the stomach, especially by the veins, alcohol is also absorbed by the large and small intestines, but in smaller proportions. When mixed with water in the proportion of 12 per cent., alcohol produces local effects in the rectum, while it does not affect the stomach when taken in the proportion of 20 per cent. When sufficiently diluted, alcohol is absorbed hypodermically more readily, and with greater effect, than when given by the mouth. It is absorbed by the serous membranes; and still more readily, when properly diluted, by the air-passages. They found that alcohol is exhaled in extremely minute quantities by the lungs. The smallest amount of elimination perhaps takes place through the skin; but probably other products of its transformation, as acetic acid, are eliminated by this channel. They found alcohol in the urine when collected *for several consecutive days*, but in small quantities. It was found that alcohol did not pass off with the bile; thus showing the groundlessness of the hypothesis that alcohol is capable of dissolving biliary calculi. According to Schulinus and others, much unchanged alcohol is found in the blood and in the organs four hours after its absorption, and the conclusion is that the greater part of the alcohol undergoes transformation within the organism.

The authors regard as exaggerated the opinions of those who consider that alcohol produces much change in the plasma or on the corpuscles of the blood. They consider from their experiments, that even when marked cerebral phenomena of alcoholism have appeared, sensation is often fairly well preserved. They affirm that alcohol gives rise to dilatation of the capillaries, and to increase in the energy of the cardiac contractions. They conclude from numerous experiments that, in a healthy man, alcohol in a physiological dose does not produce a *constant* modification of the heat-making process. In animals (dogs, guinea-pigs, birds) alcohol always lowers the temperature.

As a nervine, alcohol is very powerful, and excites the muscular nerves; and by its physical and chemical properties it limits the process of organic decomposition just as it does outside the organism.

It is shown to be probable that in one of the phases of its transformation alcohol may furnish acetic acid, but they do not believe that oxalic and formic acids are products of its oxidation on the system. The authors regard as probable the partial transformation of alcohol into aldehyde—a view which was supported by Duchek, but opposed by Lallemand, Perin, and Duroy. The results of certain experiments lead to the suspicion that the liver is the seat of the transformation of alcohol into aldehyde.

In the production of mechanical force, alcohol, besides its calorifacient properties, has a great superiority over similar aliments. With a smaller amount of combustible matter, and in less time, it produces more force. Alcohol acts, not only as a thermo-dynamogenous aliment, but also as a nervine aliment, so far as it remains unchanged. Alcohol does not furnish material for the nutrition and repair of tissues. Hence, without a proper supply of nitrogenous food, the muscles and the heart undergo degeneration, and herein lies the explanation of the good and the bad effects attributed to alcohol.

Heubach has recently confirmed the opinion that, in febrile affections, but little, if any, of the alcohol administered reappears in the urine; and Schmidt (*Centralblatt für Chirurgie*, No. 23, 1875) has endeavoured to find how much of the spirit is contained in the expired air. In no case could evidence of more than a trace of alcohol be obtained.

In the *Lancet*, 31st July 1875, is an interesting paper by Staff-Surgeon Mulvany on alcohol as a generant of thermo-electric currents in the system.

Schwalbe, of Weinheim, has recently used injections of simple alcohol in various forms of indolent glandular swellings with very favourable results. He considers that it establishes a state of chronic inflammation in the connective tissue, causing it to contract, and thus subjecting the vessels to pressure, and effecting the obliteration of the lymphatics. He reports the cure of fatty tumours by the use of such injections, to which ether was added to dissolve the fat. He finds the most important application of his plan in the treatment of cancer, by preventing its extension to the neighbouring tissues and lymphatic glands. The tumour is first to be isolated, as it were, by causing the connective tissue on all sides of it to become shrivelled. Then the contracted connective tissue approaching the growth itself presses on it, cuts off its blood supply, and so causes it to disappear by atrophy. Schwalbe, with Dr Hasse, claims to have cured three cases of cancer of the breast in this way. —*New Remedies*, July 1875.

In the *Philadelphia Medical Times*, 10th April 1875, Dr J. C. Morris gives the following rules for the administration of alcohol, except in cases of shock or hæmorrhage:—1. It should be given in small definite quantities, at intervals of three or four hours generally, so as to produce repeatedly the primary effects, and avoid the second and third stage of its action. 2. It should always, if possible, be accompanied by food. 3. It should be gradually, but steadily, withdrawn as the patient regains strength, and no patient should be left to the continued use of it after recovery.

The Physiological Action of Aldehyde.—Drs Albertoni and Lussana of Padua, to whose researches on alcohol we have already alluded in this report, have also studied the action of aldehyde. This substance agrees in its elementary properties with those of alcohol. It coagulates albuminous substances, and is a solvent of all such substances as are soluble in alcohol. It possesses powerful antiputrescent properties. Its physiological action on animals is very powerful. From three to five grammes, diluted in water, and injected into the veins of a dog, almost immediately arrest the respiration, and produce a condition of coma. The heart's action is only slightly weakened, but the muscles are paralyzed, and sensation and reflex excitability are destroyed temporarily. The animals, however, usually recover in a short while. In small doses, aldehyde produces intoxication similar to that of alcohol, but sensation is completely lost. When given by the stomach, it produces intense irritation of that organ, and gives rise to gangrene of the mucous coat, owing to its caustic properties. Large quantities of it can be inhaled through the lungs. Small quantities of it accelerate the respiration, but large ones arrest it completely, but scarcely affect the cardiac contractions. During the stage of asphyxia, irritation of the pneumogastrics exerts no influence on the heart; but when the breathing recommences, those nerves recover their irritability. Aldehyde diminishes the temperature of the body in proportion to the dose given. The vessels of the brain, observed through an opening made in the skull, are dilated at the beginning of inhalation of aldehyde, but they contract when the animal becomes narcotized. The property which aldehyde possesses of arresting the respiration, and its irritating action, will probably prevent its employment as an anæsthetic in surgical operations.

Solution of Alkaloids in Oil.—In a note in the *Pharmaceutical Journal*, 11th Sept., Mr J. B. Barnes suggests the use of glacial acetic acid as a means of preparing solutions of alkaloids in oil when required for liniments. These solutions may be made of any desired strength, and do not lose their transparency when mixed with chloroform and camphor.

Aloes.—The following conclusions are deducible from Dr Craig's experiments:—1. Aloin may, by exposure to the air, undergo considerable chemical change without losing its physiological action as an active aperient. 2. The *resin* of aloes, when thoroughly exhausted of aloin, possesses no purgative properties, and therefore cannot be the active principle of aloes. 3. The resin of aloes is not the cause of the griping which sometimes follows the administration of the drug. 4. Aloin is an active aperient, and is, in all likelihood, the active principle of aloes.—*Pharmaceutical Journal*, 7th April, and *Edinburgh Medical Journal*, June.

CARBAZOTATE OF AMMONIA AS A SUBSTITUTE FOR QUININE.—Dr Slane of Goalpara finds carbazotate of ammonia to be a very efficient remedy in the treatment of ague. It does not increase the appetite, but like quinine, after a few doses, it produces tinnitus aurium, and slight deafness. The urine becomes tinged of a deep orange colour, and occasionally the skin and conjunctiva acquire a jaundiced hue, but the discoloration soon disappears. It may be given in pill, with extract of gentian, and combination with a quarter of a grain of extract of nux vomica seems to increase its efficacy. Being perfectly soluble in water, it may be given in solution, but it has an intensely bitter taste. The largest quantity Dr Slane ever gave was 8 grains in the twelve hours.—*Indian Med. Gazette*, 1st April.

BROMIDE OF AMMONIUM.—Dr J. M. Da Costa concludes, from his experience of this agent in rheumatism, that it relieves the pain of this affection, that it acts generally upon the skin, keeps up the action of the kidneys, and lessens the tendency to internal inflammation. It does not give rise to the pallor and anæmia, general distress, and alteration of blood and tissues, which a prolonged alkaline treatment will cause. He associates with it acetate of potash when the influence of a more active diuretic is indicated. When the acute symptoms subside, he follows it up by the administration of quinine, which has, he believes, the power to lessen the tendency to relapses.—*Medico-Pharmaceutical Abstract and Review*, 1st September.

CHLORIDE OF AMMONIUM IN NEURALGIA.—In the *Practitioner* for December, Dr David Young of Florence relates two cases ot neuralgia—one of the right ovary, and the other of the fifth pair—which present several points of interest, and illustrate the value of this salt in the treatment of these affections. He points out that the chloride of ammonium, if given in larger doses than fifteen grains, is apt to cause nausea, and in some cases diarrhœa; and, moreover, if administered at distant intervals—say of three or four hours—the effect is much less marked than when given rapidly, and in anticipation of the onset.

THE INFLUENCE OF NITRITE OF AMYL IN COUNTERACTING THE DANGEROUS EFFECTS OF CHLOROFORM.—Mr C. Bader states (*Lancet*, 8th May) that in three or four seconds after taking three drops of nitrite of amyl on sugar, the bloodvessels of the retina (chiefly the veins) become enormously dilated and gorged with blood, leaving no doubt as to simultaneously existing cerebral hyperæmia, with accelerated circulation of blood. After observing upon himself the effects of inhalation of vapour of nitrite of amyl, Mr Bader conceived that in cases of faintness, or of defective breathing, or defect of the heart's action, while under the influence of an anæsthetic, inhalation of the vapour of this drug might be of use. Mr Bader publishes several cases illustrative of the value of his suggestion. So long ago as 11th June 1870, Dr F. A. Bunall, in the *New York Medical Gazette*, recommended the use of nitrite of amyl in chloroform poisoning.

Nitrite of Amyl in Nervous Cephalalgia.—Dr R. A. Douglas-Lithgow states that in this affection he has never known the inhalation of nitrite of amyl to fail to produce entire and almost immediate relief. He places two drops on the palm of the patient's hand, and, quickly diffusing these with his finger over the palmar surface, he tells her to cover her mouth and nose with her hand, and to inspire deeply and quietly. The patient should be seated while inhaling. In several instances he has had to increase or repeat the dose, but, as a rule, two drops have been sufficient. Should any untoward symptoms occur, cold affusion to head, face, and chest, with a free supply of air, should be resorted to; but he has never had occasion to adopt these measures, and, used as he recommends, Dr Douglas-Lithgow does not think there is the slightest risk in its administration.—*Lancet,* 16th October.

Nitrite of Amyl in Facial Neuralgia.—Dr George H. Evans, in the *Practitioner* for September, relates several cases illustrative of the efficacy of inhalation of the vapour of nitrite of amyl in those cases of facial neuralgia, which are obviously connected with, if not dependent on, anæmia.

Nitrite of Amyl in Sea-Sickness.—In the *Lancet* for 21st August, is a brief paper on this subject by Dr Crochley Clapham. He tried the remedy altogether in 124 cases, and of these, 121 proved eminently satisfactory, there being no return of the vomiting after the administration of the nitrite; the remaining three cases being only unsatisfactory in so far as they required a further dose or two of the remedy. His mode of administration is by inhalation, three drops of the drug being poured on a handkerchief, and held close to the patient's nose.

Arnica in Orchitis.—Mr H. G. Knaggs recommends for the cure of orchitis, first the constant application of a lotion of one part of tincture of arnica to six of water to the part; secondly, the application of an embrocation composed of one part of tincture ot arnica to one or two parts of soap liniment, twice or thrice daily along the course of the spermatic cord; and, thirdly, the internal administration of seven-drop doses of tincture of arnica combined with two-and-a-half-drop doses of Fleming's tincture of aconite, together with acetate of ammonia, if there be febrile disturbance. This treatment usually cures in a fortnight or less. A careful watch should be kept lest any symptom of erysipelatoid inflammation should occur.—*Brit. Med. Journ.*, 17th July 1875.

During the past year, attention has been directed by several American and French physicians to the noxious properties which arnica occasionally exhibits when used as an external application. Dr C. Phillips, in his Materia Medica and Therapeutics, ascribes these evil consequences to the employment of an alcoholic, instead of an aqueous, solution; which latter contains neither arnicine nor the oil. He states that he has never seen inflammatory symptoms follow the application of a purely aqueous lotion to wounds or bruises.

ATROPINE AND MORPHIA.—The following are some of the conclusions arrived at by Drs Mitchell, Keene, and Morehouse, of Philadelphia. Conia, atropia, and daturia, do not lessen pain when given subcutaneously. Morphia does so relieve pain, and is more potent in certain forms of neuralgia the nearer it is applied to the seat of pain. Morphia scarcely lowers the pulse at all, while atropia lowers it a few beats within ten minutes, and raises it from 20 to 50 beats within an hour; the pulse then falls below the normal standard about the tenth hour, and finally regains the healthy rate within the twenty-four hours. Morphia cannot prevent atropia from thus influencing the pulse, and hence these alkaloids are not antagonistic as regards the circulation. During the change of pulse under atropia, the number of the respirations is hardly altered. Atropia and morphia are antagonistic as regards the eye, but the action of atropia lasts much longer. As regards the cerebral symptoms, atropia and morphia are antagonistic, but owing to the different rates at which they affect the system, it is difficult to obtain a perfect balance of influence. The dryness of the throat caused by atropia is not relieved by morphia, and the nausea induced by morphia is not prevented by atropia. Both agents cause dysuria in certain cases, and the dysuria occasioned by one is not relieved by the other. They seem to affect the bladder similarly. Atropia does not influence the power of morphia to diminish sensibility or to relieve pain.—*Pacific Medical and Surgical Journal.*

In the *Berliner Klinische Wochenschrift,* 23d August 1875, Dr Hedler records the following case :—A child, aged 15 months, drank about half of a bottle containing more than half a grain of atropia in solution. The case was remarkable on account of the fact that no important symptoms manifested themselves for four hours. Then, however, the rapidity of the pulse increased, urgent convulsions ensued, and the scarlatina-like rash covered the head and chest. The pupils were now much dilated. A sixth of a grain of morphia was ordered in four doses to be given at intervals of a quarter of an hour. This produced no effect and two subcutaneous injections of 1-7th grain of morphia each were administered. After the second injection the spasms ceased; the child slept well until next day, and two days after there remained no bad symptom. Nothnagel will not allow that morphia is antidotal to atropia, but it seemed to act as if it was so in this instance.

In the *Indian Medical Gazette*, 1st July, G. Chunder Roy reports two cases of opium poisoning successfully treated by subcutaneous injections of atropine.

Dr Forcauld (*Mouvement Médical*, 22d May) extols the administration of injection of a mixture of morphia and atropia in the dyspnœa of phthisical patients. The following are his conclusions:—The mixed injection (equal parts of solution of hydrochlorate of morphia, 1–30, and solution of sulphate of atropia, 1–100) presents all the advantages of the injection of sulphate of atropia without its inconveniences. Six drops of the mixed injection give as good results as fifteen drops of solution of muriate of morphia. As after the employment of either the morphia or atropia alone, so after the use of the mixed injection, there is a diminution in the number of respiratory movements, in the degree of temperature, and in the rapidity of the pulse.

Dr Curtis Smith, writing in the *Detroit Review of Medicine* for July, states that atropine is almost the only reliable respiratory stimulant with which he is acquainted. Under its influence the respirations, according to him, are increased from six to twelve per minute above the normal standard. The blood thus goes to the nerve-centres in increased quantity, and with increased purity and stimulating properties. Atropia diminishes abnormally increased secretions of mucous membranes, and has become one of our standard remedies in excessive diaphoresis. In certain cases of nervous debility characterized by sluggishness of the capillary circulation entailing partial congestion of the lungs, atropia is indicated as being well calculated to stimulate the nerve-centres which control the circulation and respiration. Atropia is of great value in arresting profuse alimentary discharges. As an adjunct to laxative pills, it renders their action more certain and thorough by stimulating the nerves supplying the muscular and mucous coats of the bowels. In cases of fæcal obstructions, it is a valuable agent, and may be given when the further use of cathartics is contraindicated, as, for instance, when the bowels have become inflamed.

Part Fifth.

MEDICAL NEWS.

Do Medical Men Live Long?—This to many of us may be an interesting question, and it is one of vast importance to all insurance offices connected with the medical profession.

From a most interesting table, published in the *Lancet* of 15th January 1876, in illustration of Dr Richardson's first lecture on Industrial Pathology, we find the startling result that, using as a standard males of all occupations, for every 100 of such who die, 106 physicians and surgeons may be expected to die; while in the same period 63 barristers, 71 clergymen of Church of England, 75 Protestant clergymen, and 80 gamekeepers will die. On the other hand, 110 commercial travellers, 115 miners, 121 railway servants, 138 publicans, and, last and most fatal occupation, 143 cabmen.

An inspection of the table, however, points out the very interesting fact, that it is chiefly among the younger members of the profession that the mortality is so great: exposure to infection, hard work, small pay, produce the bad effects; the death-rate per 1000 living after the ages of 55 and 65 being considerably less than the average; and we see by the statistics of the medical men of eminence who have lately died, to what a great age some of the hardest-working and successful men have attained. From the *Lancet* of 8th January 1876, we quote the following:—

"The profession of medicine and the cares of a medical practice cannot be said to be conducive to longevity. The tear and worry incidental to anything like an active pursuit of the calling is far greater than that to which members of the other popular professions —the church and the bar—are subject. Nevertheless, we often find gratifying illustrations of how the possessor of a good constitution, who practises sobriety and regularity of living, will overcome all the influences which tend to shorten his life. A notable year for instances of longevity in our own ranks is that which has just passed away. The following physicians and surgeons died during the year, at the ages indicated—viz., James Dawson, F.R.C.S. Eng., 96; Peter Mere Latham, M.D. Oxon., F.R.C.P., Physician Extraordinary to the Queen, 86; R. Stanley Ireland, M.D. Dub., Senior Fellow of the Royal College of Surgeons of England, 96; Arthur Helsham, M.R.C.S. Eng., 90; Sir Charles Locock, Bart., M.D., D.C.L., F.R.S., 77; William Beattie, M.D., F.R.C.P. Lond., 82; George Webster, M.D. and J.P. for Surrey, 79; H. M. Keble, M.D., Staff-Surgeon, R.N., 84; William Hey, F.R.C.S. Eng., of Leeds, 79; James Snow, F.R.C.S. Eng., J.P. for Lincoln, 96; Thomas Paget, F.R.C.S. Eng., of Leicester, 79; Henry Franklin, C.B., F.R.C.S. Eng., Inspector-General of Hospitals, 89; Robert Law, M.D. Dub., 76; John Dobson, M.D. Edin., 85; Andrew Morrison, M.D., R.N., 89; William Macdonald, M.D., F.R.S. Edin., 84; Francis Kiernan, F.R.S., F.R.C.S. Eng., 76; H. S. Murley, M.D., 88; Henry S. May, M.D., Army, 86."

AMERICAN CENTENNIAL CELEBRATION. INTERNATIONAL MEDICAL CONGRESS.—The Medical Societies of Philadelphia, animated by a just spirit of patriotism, and an earnest desire to unite with their fellow-citizens in celebrating the Centennial Birthday of American Independence, have taken the initiatory steps for the formation of an International Medical Congress, by the appointment of delegates from their respective bodies, who were empowered to organize and perfect a scheme for the above purpose. In accordance with the authority thus given, the delegation has organized the Centennial Medical Commission, with the following officers:—President, Samuel D. Gross, M.D., LL.D., D.C.L. Oxon. Vice-Presidents, W. S. W. Ruschenberger, M.D., U. S. N., Alfred Stillé, M.D. Recording Secretary, William B. Atkinson, M.D. American Corresponding Secretaries, Daniel G. Brinton, M.D., William Goodell, M.D. Foreign Corresponding Secretaries, Richard J. Dunglison, M.D., R. M. Bertolet, M.D. Treasurer, Caspar Wister, M.D. Arrangements have been made for the holding of the congress in the city of Philadelphia, to begin on the 4th, and to terminate on the 9th of September 1876. The Commission propose the following general plan for the organization and business of the Congress:—1. The Congress shall consist of delegates. American and foreign, the former representing the American Medical Association and the State and Territorial Medical Societies of the Union; the latter the principal medical societies of other countries. 2. The officers shall consist of a President, ten Vice-Presidents, four Secretaries, a Treasurer, and a Committee of Publication, to be elected by the Congress at its first session, on the report of a Committee of Nomination. 3. The morning sessions of the Congress shall be devoted to general business and the reading of discourses; the afternoons to the meetings of the sections, of which there shall be nine, viz.:—1. Medicine, including Pathology, Pathological Anatomy, and Therapeutics; 2. Biology, including Anatomy, Histology, Physiology, and Microscopy; 3. Surgery. 4. Dermatology and Syphilology; 5. Obstetrics and Diseases of Women and Children; 6. Chemistry, Toxicology, and Medical Jurisprudence; 7. Sanitary Science, including Hygiene and Medical Statistics; 8. Ophthalmology and Otology; 9. Mental Diseases. 4. The language of the Congress shall be the English, but not to the exclusion of any other language in which members may be able to express themselves more fluently. Gentlemen intending to make communications upon scientific subjects will please notify the Commission at the earliest practicable date, in order that places may be assigned them on the programme. In order to impart to the Congress a thoroughly international character, invitations to send delegates will be extended to all the prominent medical societies in Europe, Mexico, the British Dominions, Central and South America, the Sandwich Islands, the East and West Indies, Australia, China, and Japan. Invitations will also be tendered to medical gentlemen of high scientific position; and distinguished visitors may be admitted to membership by a vote of the Congress. Among the advantages arising from such a convocation as this, not the least important will be the opportunity afforded its members for the interchange of friendly greetings, the formation of new acquaintances, and the renewal and cementing of old friendships. The Centennial Medical Commission tender in advance to their brethren in all parts of the world a cordial welcome, and a generous hospitality during their sojourn in the "Centennial City." The Congress will be formally opened at noon, on Monday, the 4th day of September 1876. The registration book will be open daily from Thursday, 31st August, from 12 to 3 P.M., in the Hall of the College of Physicians, N.E. corner 13th and Locust Streets. Credentials must in every case be presented. Gentlemen attending the Congress can have their correspondence directed to the care of the College of Physicians of Philadelphia, N.E. corner of Locust and 13th Streets, Philadelphia, Pennsylvania. There is every reason to believe that there will be ample hotel accommodation for all strangers visiting Philadelphia in 1876. Further information may be obtained by addressing the Corresponding Secretaries. All communications must be addressed to the appropriate Secretaries. William B. Atkinson, 1400 Pine Street, Philadelphia, Recording Secretary; Daniel G. Brinton, 2027 Arch Street, and William Goodell, 20th and Hamilton Streets, American Corresponding Secretaries; Richard J. Dunglison, 814 N. 16th Street, and R. M. Bertolet, 113 S. Broad Street, Foreign Corresponding Secretaries.

OBITUARY.

MR JAMES HINTON, M.R.C.S.E., formerly Aural Surgeon to Guy's Hospital, London, and author of two of our best works on the ear and its diseases, viz., "The Questions of Aural Surgery," and "The Atlas of Diseases of the Membrana Tympani," died on the 16th December, at St Michael's, Azores, whither he had gone to recruit his health, at the early age of fifty-four. Mr Hinton was the son of the Rev. J. Howard Hinton, once a distinguished orator in the Baptist denomination. The deceased was a great authority on physiology, and a well-known writer in the *Contemporary Review* and other periodicals. Of the works on Aural Surgery, mentioned above, a highly favourable review is given in the *Archiv für Ohrenheilkunde* of 17th December, last year, the author of which expresses his deep regret that, on account of failing health, they would be Mr Hinton's parting gift to otology. Mr Hinton was also the author of "The Place of the Physician," being the introductory lecture at Guy's Hospital, session 1873-4; "Physiology for Practical Use," by various authors, edited by Mr J. Hinton; "Life in Nature;" "Man and his Dwelling-Place;" "Thoughts on Health, and some of its Conditions;" "The Mystery of Pain: a Book for the Sorrowful," etc., etc. He earned a high reputation and a fortune as an aural surgeon in Savile Row, and then devoted himself to philanthropic and studious subjects. Mr Hinton has been succeeded at Guy's by a distinguished graduate of Edinburgh University, Dr Laidlaw Purves.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Refracture in Cases of Deformity from badly-adjusted Fracture.* By JAMES SPENCE, F.R.S.E., F.R.C.S.E., Surgeon in Ordinary to the Queen in Scotland, Professor of Surgery in the University of Edinburgh.

REFRACTURE of a badly-united broken bone can scarcely be regarded as a novelty in surgical practice, for in early surgery, especially amongst the Arabian physicians, it seems to have been frequently practised; so much so indeed, as to call forth a strong protest from Abulcasis, who reprobated the frequency with which surgeons had recourse to it, and, at the same time, expressed his preference for resection of the fracture, which he speaks of as a simple and successful method of treatment. Mr John Bell, writing on this subject, says, "It was at one period so common an operation, that it looks indeed as if they had broken legs out of mere caprice. Here I cannot help remarking, that wherever there existed hot wars betwixt the bone-setters and the regular surgeons, this practice prevailed in a particular manner. The surgeons, by laying a leg across a stool or across the knee, and breaking it, taught the country people what sort of punishment awaited those who ventured to put themselves into the hands of professed bone-setters; and one example of this kind now and then served to keep a whole province in awe, and suppressed all petty treasons against the regular professors of surgery." It was, however, only in recent cases that this bending or breaking badly-set fractures was had recourse to, for we find Heister stating that it is not to be done "but where the callus is tender, and the patient young and vigorous;" and in cases of recently united fractures attended with deformity, the practice of remodelling, or, if necessary, refracturing the limb, by bending it over the knee or over a chair or block, has continued to be occasionally resorted to. In cases of fractures of long standing, attended with great distortion and shortening, in which firm union had been long completed, attempts to refracture the limb seem to have been deemed hopeless, on account of the degree of force required to accomplish the object in view, the probability of the refracture not taking place at the proper part, and the risk of injury to the large bloodvessels or nerves in the neighbourhood. When attempts to remedy such deformities have been made, section by various forms of osteotomes seems to have been preferred—a method not free from special dangers, not easy of performance, nor yet very successful in cases where much deformity and overlapping exist.

Mr Butcher of Dublin, in a number of his Reports on Operative Surgery, published in 1874, drew attention to the subject by recording a case of great deformity resulting from fracture of the femur, which had thoroughly consolidated about five months before he saw the patient. The young man was mate of a ship, and had met with the accident at sea by falling from a height of 40 feet. He sustained a bad fracture at the middle of the femur. The ship put back into Liverpool, and he was placed in hospital, but the injury resulted in great shortening and distortion of the limb; and on account of this, he consulted Mr Butcher, who, after considering all the risks, determined to refracture the thigh by means of a powerful osteoclast. The result was a most perfect and triumphant success. It so happened that I was considering what could be done in the case I am about to narrate, when Mr Butcher's paper reached me. I communicated with Mr Butcher, who agreed with me that though the case of my patient was a most unfavourable one for anything being done, and a very severe test for his method of treatment by refracture, yet that seemed to afford the best chance of at least diminishing the extreme deformity. The following history of my case is taken from the Hospital journal :—

CASE OF REFRACTURE OF FEMUR FOR A BADLY-UNITED FRACTURE.

Thomas I., æt. 24, unmarried, a farmer, residing in Ottawa, Canada, was admitted to Ward XIII., S., on 8th October 1874, for badly united fracture of the femur.

History.—Twenty-one months ago, when up country, his thighbone was broken by a tree falling upon it. It was three days before it was set, and then merely short splints were applied. He lay in bed for three months but as the limb was never seen to after being set, the bone united very badly.

On admission there was a marked deformity and shortening of the limb. It was four inches shorter than the right; whilst at the junction of the middle and lower thirds of the femur there was a great knee of bone projecting outwards, caused by the upper portion of bone riding on the lower. The knee was thrown outwards and forwards, the foot everted, and when the patient was in the upright position, it did not touch the ground. The muscles on the front and outside of the leg were paralyzed, owing to the pressure of the bone on the external popliteal nerve.

9th November.—The extension apparatus was applied shortly after admission, but did little good. The tendo Achillis was then divided, and the interrupted galvanic current applied to the paralyzed muscles. The electro-contractility and sensibility were both found to be diminished in the injured leg.

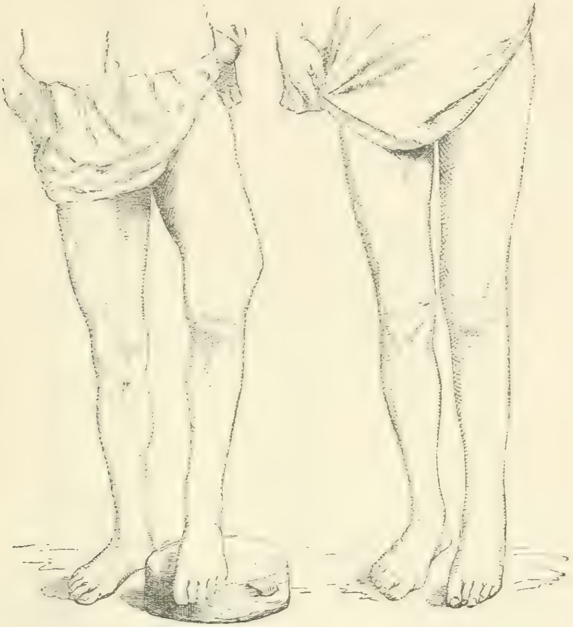

Fig. 1.

30*th March* 1875.—Since last entry little was done beyond the application of the interrupted current thrice a week.

31*st March*.—To-day, the patient having been placed under chloroform, Butcher's osteoclast was carefully adjusted to the limb, so that the pad lay over the projecting piece of bone. The instrument was then screwed home. A slight creak was heard during this, but as there appeared no sudden giving way of the bone, the sound was believed to proceed from the instrument. It was not until the screw was undone that the femur was found refractured obliquely, just where the union had taken place. Traction was now made on the limb, the long splint applied in the usual way, and well-padded Gooch's splints placed around the thigh to maintain the bones in position. The extension apparatus, weighted with 11 lbs., was also used to assist in extending the limb.

2*d April*.—Pulse and temperature have been normal since the operation, except on that evening, when the temperature rose to 100°. Other 2 lbs. added to extension.

15*th April*.—The long splint and perineal band were removed ten days ago. The extension weight has been gradually increased, so that now it amounts to 21 lbs. To-day the patient was put under chloroform, and forcible extension made on the limb; ¼ inch was thus gained, and it is now only 1⅞ inch shorter than its fellow.

30th April.—Patient's general health continues, as it has been throughout, perfectly good. He complains of pain on pressure on the outer aspect, at the seat of fracture; and, consequently, no further extension is to be attempted in the meantime.

6th June.—The extension apparatus (weighted with 15 lbs.) has been resumed for a time. Limb now 1¼ inch short.

26th June.—Patient allowed to get up.

15th July.—Sent to Convalescent House.

2d Aug.—Readmitted.

10th Aug—Went home to Canada. The limb is quite straight, but the extensors of foot and peronei are still paralyzed.

As Mr Butcher's paper on refracture may not be easily obtained now, I think it advisable to add the accompanying illustration and description of the osteoclast and the method of applying it.

The apparatus consists of a solid piece of wood 15 inches long, 10 inches wide, and 3½ or 4 inches in depth. Into the centre of this block a strong bar of steel is firmly implanted, and then rises perpendicularly at one side for 12 inches, and is then again curved over towards the centre of the block of wood, and terminates in a female screw, 2 inches deep; through this a male screw plays. The male screw is fully 12 inches long, with an expanded plate of metal of bell shape at its lower end, in which the end of the screw turns; through an opening at its upper end a strong rounded bar of steel 6 inches long passes, which is used as a lever to turn home the screw. When the operation is to be performed, the long screw is elevated fully, the limb laid on the block of wood, as represented in Fig. 2, and then a piece of wood, 2 inches thick, 6 inches long, and 3 wide, well cushioned, is laid over the most prominent or deformed part, and held steadily by an assistant. The screw is then brought down on the small block of wood so as to fix it firmly, and by gradually bringing down the screw, whilst the limb is kept steady, sufficient force is employed to refracture the bone; then by forcible steady extension, whilst the screw is still gently worked down, the deformity is remedied. The limb is then placed either in such a fracture-box as recommended by

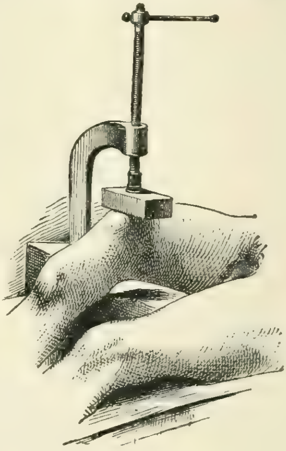

Fig. 2.

Mr Butcher, or treated by the ordinary extension apparatus. The accompanying illustration will give a better idea of the apparatus and its application than any verbal description.

The case narrated above presents some points of interest in reference to the treatment of such badly-united fractures. As regards the probabilities of success, it would be difficult to conceive a case more unfavourable. The length of time which had elapsed since the injury, the amount of overlapping and deposition of new bone, which appeared to be due to some detached comminuted portions, the great distortion and lateral projection, together with the contraction and alteration of the muscular and other tissues of the injured limb, were very serious obstacles to refracture and extension of the limb being readily accomplished. And then there was the very hopeless feature of paralysis of the extensor and peronei muscles, too obviously due to lesion of the external division of the popliteal nerve at the time of the injury, and the great retraction of the heel, as a consequence of the unopposed action of the gastrocnemius and soleus muscles; whilst the vitality of the leg and foot seemed considerably impaired, owing to obstructed circulation and diminished innervation from the pressure of the bony mass near the popliteal space.

The question of interference was therefore a very serious one; for besides the ordinary risks of the bone not breaking at the proper place, or of injury to the great vessels by any fragment of the bone, there were added the dangers arising from great direct force; the bruising and extravasation likely to arise from such force, applied to a limb whose vitality was so weakened; and also the risk of laceration of the contracted and consolidated soft textures of the limb in attempts at extension after refracture was effected.

The early part of the treatment by extension was with the view of exercising a gradual extension on the soft textures of the limb, to prepare for further extension after the operation.

The use of galvanism was resorted to, to try and restore the muscular contractility of the extensors, and in the hope that the nervous power might be in abeyance owing to pressure rather than absolutely destroyed, and it was obvious that the muscles in question could not act until relieved from the opposition of the gastrocnemius and soleus by division of the tendo Achillis.

Having continued the preliminary treatment for some time, I proceeded to refracture the femur. Owing to the oblique nature of the fracture, and the outward projection of the deformed part, it was more difficult to adjust the force to bear exactly upon it than if the projection had been directly forwards. Still, this difficulty was overcome, and refracture accomplished. Forcible extension and counter-extension was made by assistants, but the full length of the limb could not nearly be restored. As I was prepared for this, from the considerations I have already stated, I determined to effect extension gradually by the pulley and weight. The elongation effected was maintained by applying the long splint before the patient was removed from the theatre, and subsequently extension by weight and pulley was substituted, combined with the short splints around the thigh to obviate lateral displacement.

The result was such as I could hardly have expected. So far from the dreaded bruising, laceration, or swelling at the part where the force was directly applied, there was no ecchymosis, and scarcely any appearance of swelling, and no rise in general temperature or pulse. In fact, after the effects of the chloroform had passed off, there never was any constitutional disturbance nor complaint of pain.

The loss of nervous power was not much ameliorated, but the success in the restoration of the limb to nearly its original length, and the removal of the great deformity, may be judged of from the accompanying woodcuts from photographs of the limb before and after the operation. In conclusion, I think that such a testing case as this proves satisfactorily the efficiency and safety of Mr Butcher's method of effecting refracture.

ARTICLE II.—*On the Alleged Occasional Epidemic Prevalence of Puerperal Pyæmia or Puerperal Fever, and of Erysipelas.* By J. MATTHEWS DUNCAN, M.D.

(*Read before the Medico-Chirurgical Society of Edinburgh, 2d February.*)

I HAVE repeatedly alleged that puerperal pyæmia or fever does not occur as an epidemic, but I have never solemnly adduced evidence to that effect. This I now propose to do.

The prevalent, if not the universal, opinion still is, that both it and erysipelas do prevail epidemically at certain times. To show this, I quote a paragraph from a recent speech by Dr Priestley, the distinguished President of the Obstetrical Society of London. "A warm discussion (says he[1]) has taken place as to whether puerperal fever is ever really epidemic; that is, dependent on general atmospheric or other influences which pervade a community, as distinctive from the miasms of unhealthy hospitals, or the propagation of the disease from one patient to another by inoculation. Dr Matthews Duncan is inclined to doubt the evidence of its epidemic character altogether, and to regard it as essentially endemic. There is no doubt, as he suggests, that the word 'epidemic,' as applied to an outbreak of puerperal fever, has been at times used by authors carelessly, and that 'endemic' would have been the more appropriate term; but the annals of medicine still afford undoubted evidence of the epidemic character of puerperal fever at definite epochs, and, if we turn to the histories by Hirsch, by Dr Robert Ferguson, and others, besides the speech of Dr Fordyce Barker in this Society, we shall find that it occurred extensively in certain years among all classes of the community, in hospital and out of it, in town and country. In 1821, for example, it occurred as an epidemic in France, in Holland, in Scotland, and in London at the same time; while, further, it has been noticed that, when puerperal fever prevails epidemically in the human subject, the lower animals die in large numbers of diseases connected with parturition. I confess I cannot see what is gained in argument by those who contend that puerperal fever is essentially a form of pyæmia (using the term pyæmia with the latitude they attach to it), by denying that puerperal fever is at times epidemic, unless it be desired to prove that the disease always originates in some local inflammation of the generative organs, and that the febrile symptoms are only the natural effect of the intense constitutional effect so engendered. This theory of the localists, as it has been called, was supported by Professor Meigs, Professor Pajot, and others, but has received little countenance in this country. Erysipelas is so undoubtedly and frequently epidemic in its character, has so commonly a spontaneous or idiopathic origin, apart from its association with traumatic injuries, that, if the mutual interchange between puerperal fever and erysipelas be admitted, the epidemic character of both follows as a matter of course."

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[1] British Medical Journal, 8th January 1876. P. 36.

It will be admitted to have been the duty, albeit a neglected one, of all who have asserted the epidemic character of puerperal fever and erysipelas, either to give reason for the belief, or to refer to some sufficient evidence. Yet, though the so-called epidemics are said to be frequent, constantly being reported, no attempt has, so far as I know, been made to demonstrate the truth of the common assertion. All is mere allegation. In Hirsch, in Ferguson, in Churchill, in Fordyce Barker, there is nothing more.

It is in consequence of this lamentable defect that I feel called upon to attempt to prove the negative.

I shall say nothing of the well-known occasional or even frequent endemic character of the diseases, as specially well illustrated in ill-managed hospitals.

In conducting my argument, I shall make use of statistics. Without them nothing can be proved or disproved in this department of pathology. Recognising the numerous errors and imperfections of those which I shall adduce, I am yet sure that all ingenuous minds will admit their sufficient accuracy for the purposes of the present investigation, and the superiority of the evidence they afford to every other kind of evidence in this matter.

I shall use exclusively the statistics of London, because of their quality of unity, the largeness of the numbers, the consequent obliteration of petty disturbing influences, the comparatively very great elaboration of the data as presented in the published reports, and because of their convenient accessibility.[1]

The statistics are, as is well known, of mortality only, not of morbility—of fatal cases only, not of all cases; but no one will think their value for my present purpose to be on this account diminished. This remark is, perhaps, peculiarly appropriate to puerperal fever, which, at all times a very dangerous disease, has frequently, indeed generally, been described as specially deadly when raging epidemically.

The evidence to be adduced I shall give graphically in diagrams, which, however, it will be necessary to supplement by descriptive text. I shall show, first, that puerperal pyæmia or fever, and erysipelas, do not behave like diseases whose epidemic character is established; and, second, that they do behave like diseases which are recognised as having no epidemic character, or can be shown to have none.

In this vast city of about 3½ millions of inhabitants, it is seen, for example, in Diagram I., of yearly mortality, that cholera, scarlatina, and smallpox have raged epidemically, and made fearful ravages, while puerperal fever, erysipelas, and rheumatism have had only slight variations in their prevalence.

Cholera is sometimes absent from London. In 1860 there were only 46 deaths from it, while in 1849 there were 14,125 deaths from the same cause, or more than 300 times as many as in 1860.

Smallpox is sometimes absent from London. In 1874 there were only 56 deaths from it, while in 1871 there were 7876 deaths from the same cause, or 140 times as many as in 1874.

Scarlatina is never absent from London. It generally prevails epidemically, as I shall hereafter show. Hence, comparison of years may not show its variations so clearly as in the cases of cholera and smallpox. In 1873 there were 634 deaths from it, while in 1870 there were 5998 from the same cause, or more than 9 times as many as in 1873.

Rheumatism destroyed 268 in 1848. In 1874 it destroyed 526, or nearly twice as many as in 1848.

Erysipelas destroyed 255 in 1861. In 1848 it destroyed 579, or little more than twice as many as in 1861.

Puerperal fever destroyed 133 in 1857. In 1874 it destroyed 456, or about 3½ times as many as in 1857.

Bronchitis destroyed 3030 in 1848. In 1873 it destroyed 9908, or above three times as many as in 1848.

But these figures have an evident fallacy running through them,

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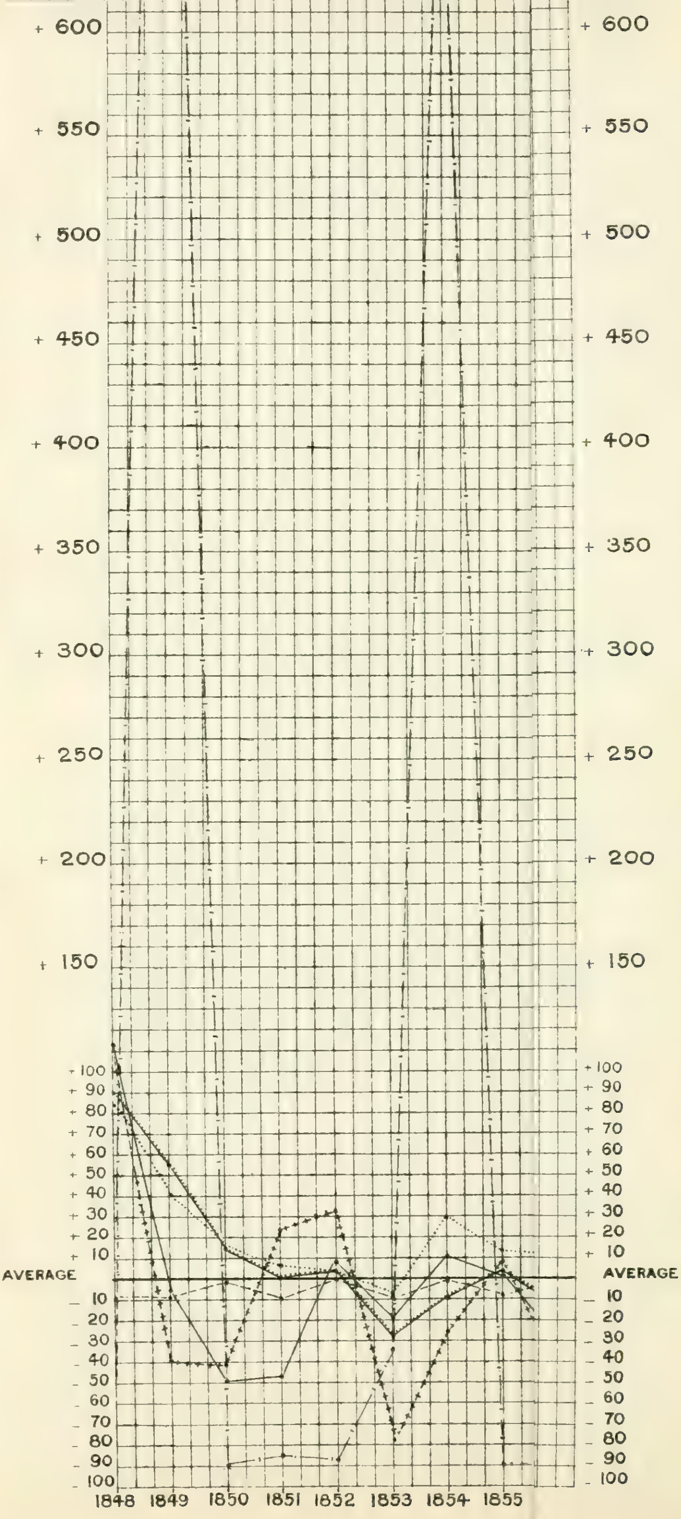
[1] I have here to record my heartfelt thanks to Mr Buchan and to Dr Arthur Mitchell for their aid in this matter. In preparing their great work on the influence of weather on mortality from different diseases, and at different ages (see the *Journal of the Scottish Meteorological Society*, 1875), they had occasion to collect all the data which I required as the basis of my paper. These data they have in a truly generous scientific spirit handed to me, and thus saved me a lengthy and wearisome course of making of extracts.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1853
1854
1855

DIAGRAM Nº I

YEARLY MORTALITY IN LONDON,

FROM

CHOLERA, SMALLPOX, SCARLATINA, PUERPERAL FEVER, ERYSIPELAS & RHEUMATISM

From 1848 to 1874 inclusive

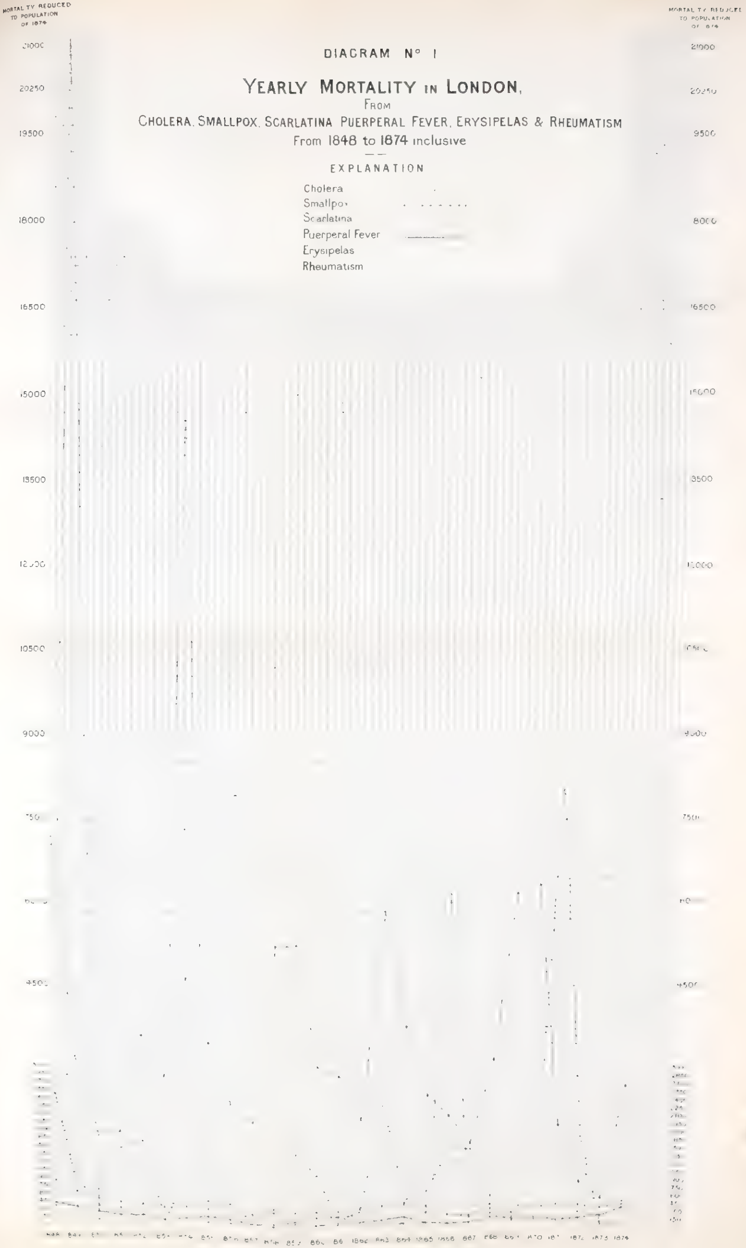


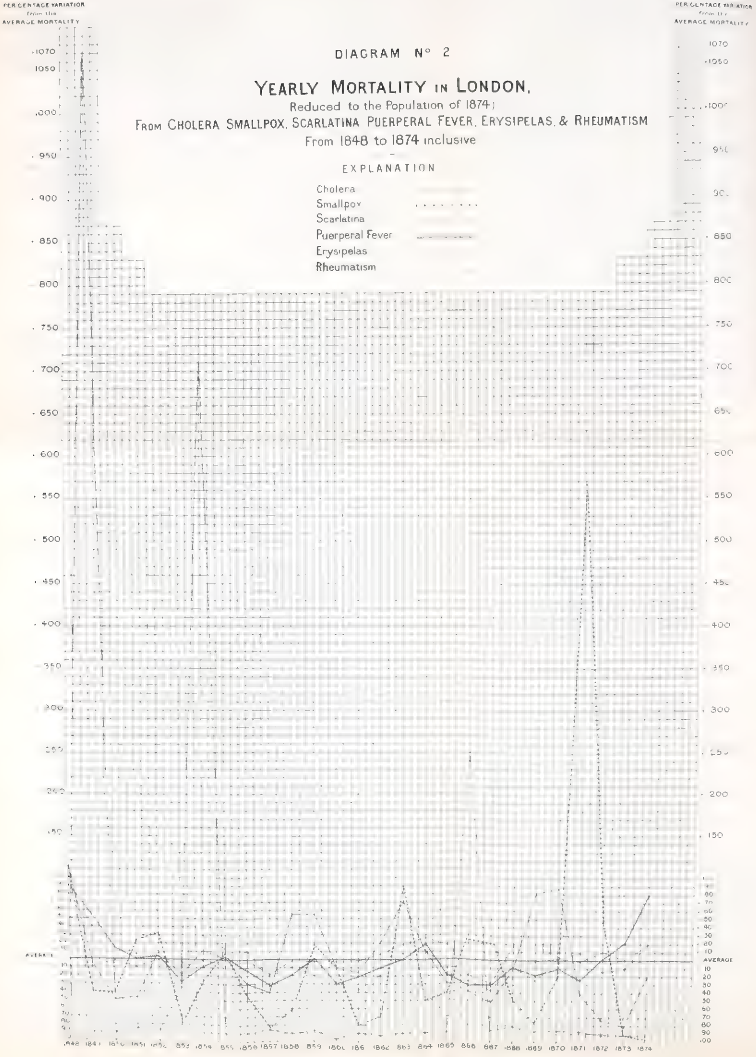
DIAGRAM Nº 2

YEARLY MORTALITY IN LONDON,

(Reduced to the Population of 1874)

FROM CHOLERA, SMALLPOX, SCARLATINA, PUERPERAL FEVER, ERYSIPELAS, & RHEUMATISM

From 1848 to 1874 inclusive



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LONDON,

f 1874)

'ER, ERYSIPELAS, & RHEUMATISM.

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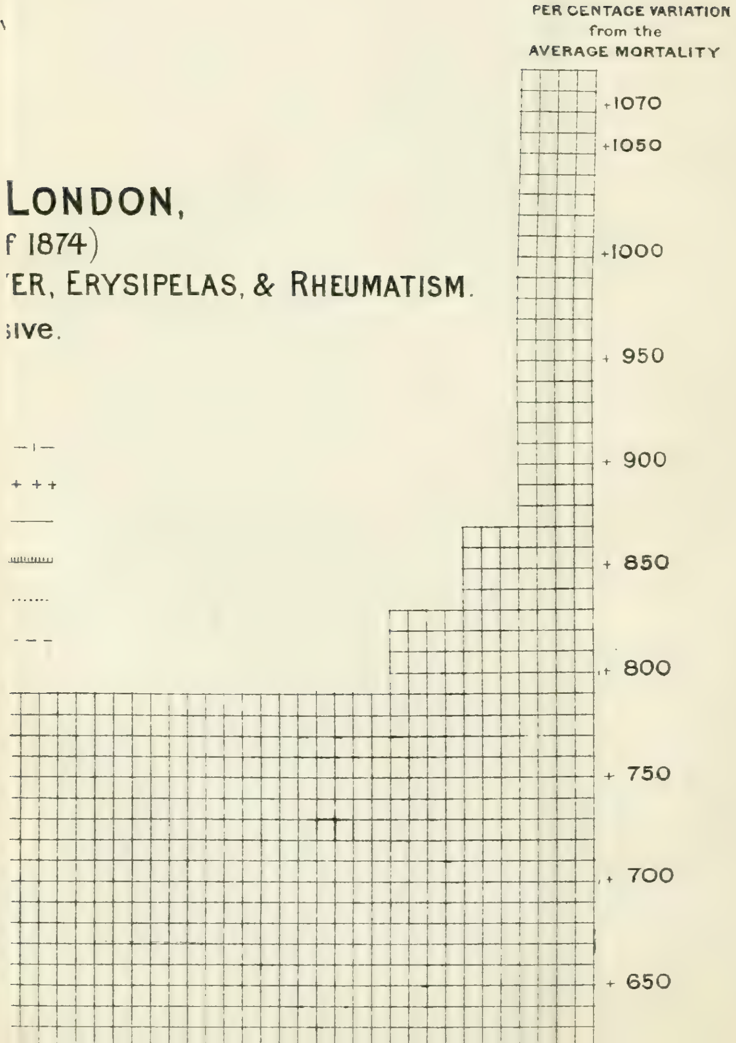

which can be corrected, as is done in Diagram I. It arises from the circumstance that London is a rapidly growing city. In 1848, the first year available for my purposes, its population is estimated as 2,244,837, while in 1874 it is estimated 3,400,701, or 1,155,864 more than in 1848. Now, it is a simple matter to correct for this growth. This I have had done, and the result is given graphically in Diagram I. There the great epidemics of cholera, smallpox, and scarlatina, are easily seen by the eminences of their outline, while rheumatism, erysipelas, and puerperal fever have only more moderate undulations. The former comparison of the highest and lowest for the years 1848 to 1876 inclusive, can now be more justly given from this diagram, and we have the following:—

TABLE I.

Showing Highest and Lowest Yearly Mortalities.

	Lowest.	Highest.	Proportion.
Cholera	57 (1860)	21099 (1849)	1 to 370
Smallpox	56 (1874)	8193 (1871)	1 to 146
Scarlatina	643 (1873)	7201 (1870)	1 to 11$\frac{1}{5}$
Rheumatism	320 (1867)	555 (1864)	1 to 1$\frac{2}{3}$
Erysipelas	293 (1867)	876 (1848)	1 to 3
Puerperal Fever	171 (1866)	486 (1848)	1 to 3
Bronchitis[1]	4608	10827	1 to 2$\frac{2}{5}$

Diagram I. and Table I. show what would be the actual figures for the highest and lowest years, had London had the same population in 1848 and in all the intervening years to 1874, as it had in 1874 actually. But the comparison of different years must be given not merely, as in the proportion column of Table I., of the years of highest and lowest mortality, but of all the years 1848 to 1874 inclusive. This comparison is given in Diagram I.

In Diagram II., the zero line stands as the mean or average mortality of each of the various diseases compared; and, with a view to destroying the imposing influence of mere numbers, the actual figures are not represented, but the percentages above and below the mean. In this way a rise from one million to two millions has the same diagrammatic representation as a rise from one to two. The diagram speaks for itself, and shows clearly a marked contrast between the behaviour of epidemic diseases, such as cholera, smallpox, and scarlatina, and diseases not epidemic, such as erysipelas and puerperal fever. This method I adhere to, because of the difficulty of attaining a perfect better one, and partly because it does injustice to my cause. For the great epidemics raise in a disproportionate way the means or averages of the corresponding diseases, and make the variations above them less than they would be, were a normal line taken, which would run through their small epidemic or non-epidemic times, and represent their usual condition. In other words, there is what may be called a normal average for the non-epidemic diseases, but there is no normal or fixed average for the epidemic diseases. These epidemics are on a quite different scale from that of the prevalence of the diseases when not epidemic.

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[1] I have added to this table the corresponding facts regarding bronchitis merely to extend the comparison.

It may be remarked that this Diagram II. reveals a closer correspondence between rheumatism and puerperal fever, than between erysipelas and puerperal fever.

I shall now omit from further consideration such diseases as cholera and smallpox, whose fearful epidemic ravages need no remark. I have introduced them, and scarlatina too, to make my view complete of the utter difference between their behaviour and the behaviour of such diseases as puerperal fever and erysipelas. To those who are, in a small degree even, familiar with such studies, the demonstration of the behaviour of epidemic diseases is not required. A good graphic view of this subject is to be found, given by Dr Stark, in the second volume of the Eighth Decennial Census of Scotland, published in 1874. There the behaviour of the whole zymotic diseases, including scarlatina, and of the chief individual zymotic diseases, including scarlatina separately, is well shown, and the contrast between them and the non-zymotic or non-epidemic diseases is well marked. It is just such as I have shown to exist between the zymotic diseases and puerperal fever and erysipelas.

The succeeding demonstration will, by further analysis, continue and increase the evidence to the same effect.

I still retain scarlatina for discussion, because the evidence derived from Diagrams I. and II. is not full enough regarding this disease, and because it is believed to have strong alliance with puerperal fever. The evidence of Diagrams I. and II. is not full enough, because a year encloses, or may enclose, more than an epidemic of scarlatina. There may be a non-epidemic prevalence and an epidemic within the limits of a year. It is not my object here to point to the many interesting observations derivable from the diagrams, but this one of the length of a scarlatinal epidemic has bearings on my subject, and must be insisted on. To contrast, therefore, fully, the prevalence of scarlatina with the prevalence of other diseases, we must have a weekly, not yearly, demonstration, which is given in Diagram III. In this extensive diagram, numerous epidemics of scarlatina are seen, and their cyclical history may be studied. No such epidemic of puerperal fever or of erysipelas is to be seen; nor any cyclical progress from small to great epidemics, ending in non-epidemic prevalence, as in the case of scarlatina.

The subject is susceptible of partial illustration by the following:—

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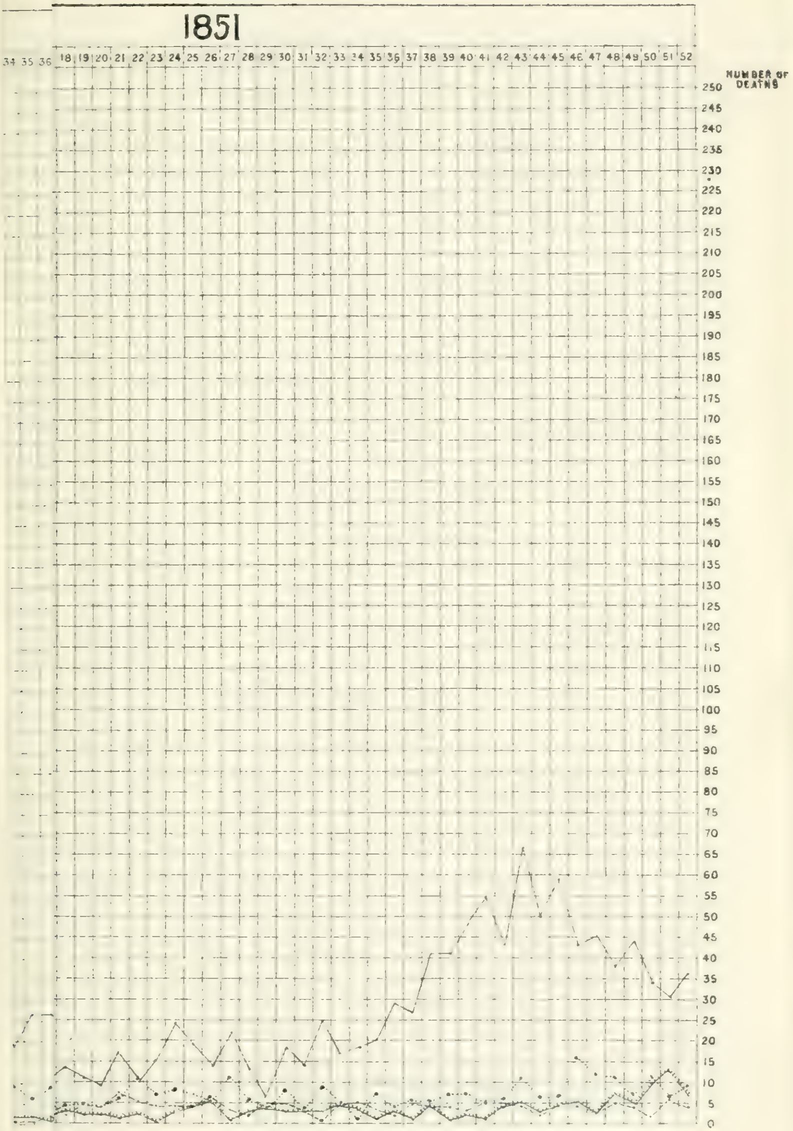
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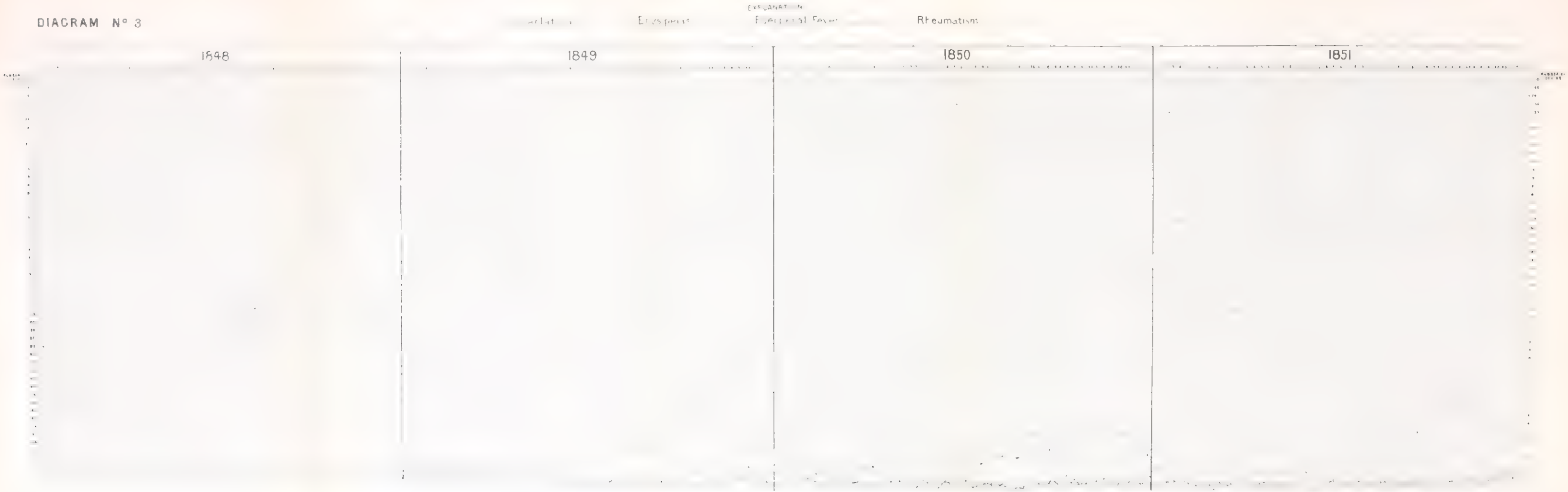
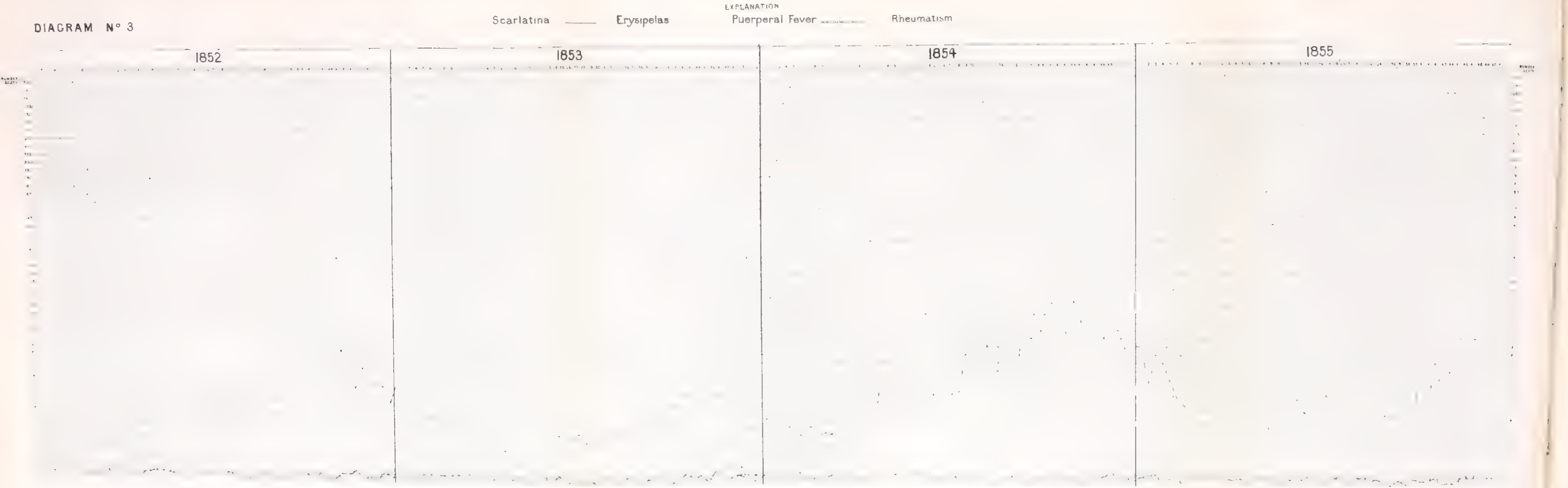
DEATHS FROM SCARLATINA PUERPERAL FEVER, ERYSIPELAS & RHEUMATISM IN EACH WEEK OF THE YEARS 1848 TO 1874 INCLUSIVE

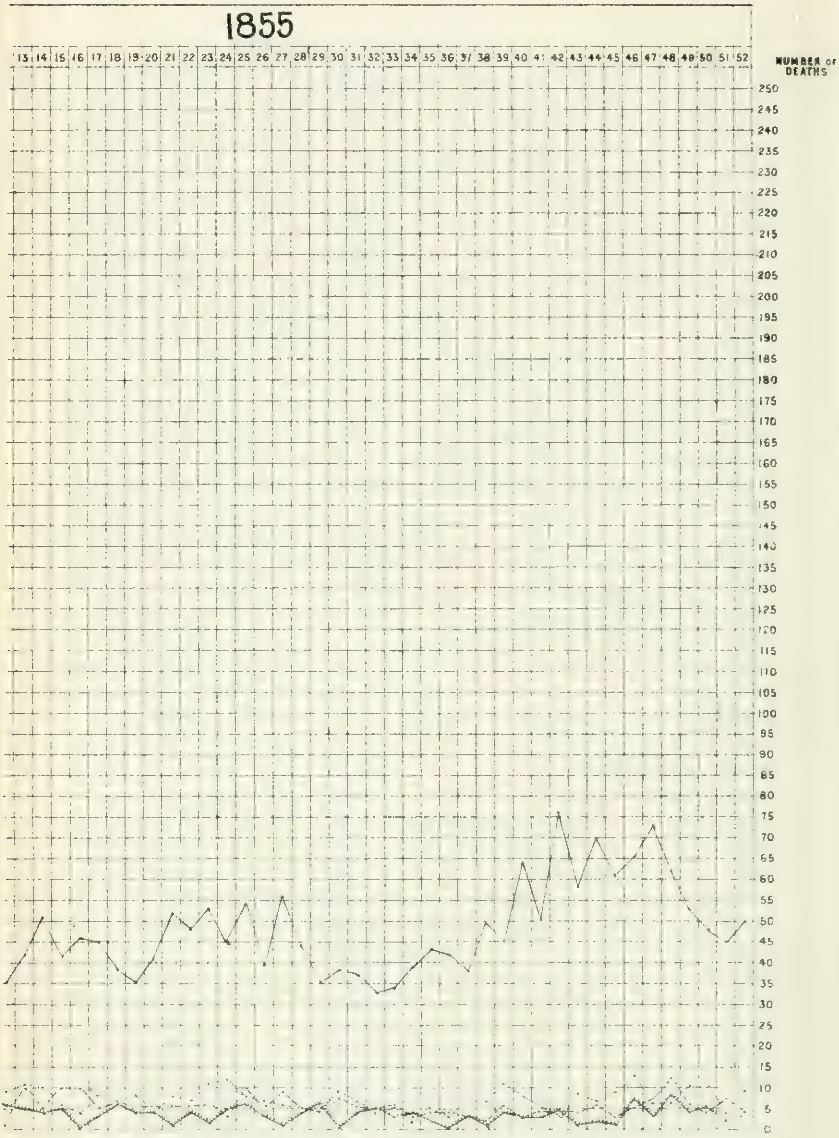
DIAGRAM Nº 3



DEATHS FROM SCARLATINA, PUERPERAL FEVER, ERYSIPELAS, & RHEUMATISM IN EACH WEEK OF THE YEARS 1848 TO 1874 INCLUSIVE.

DIAGRAM Nº 3

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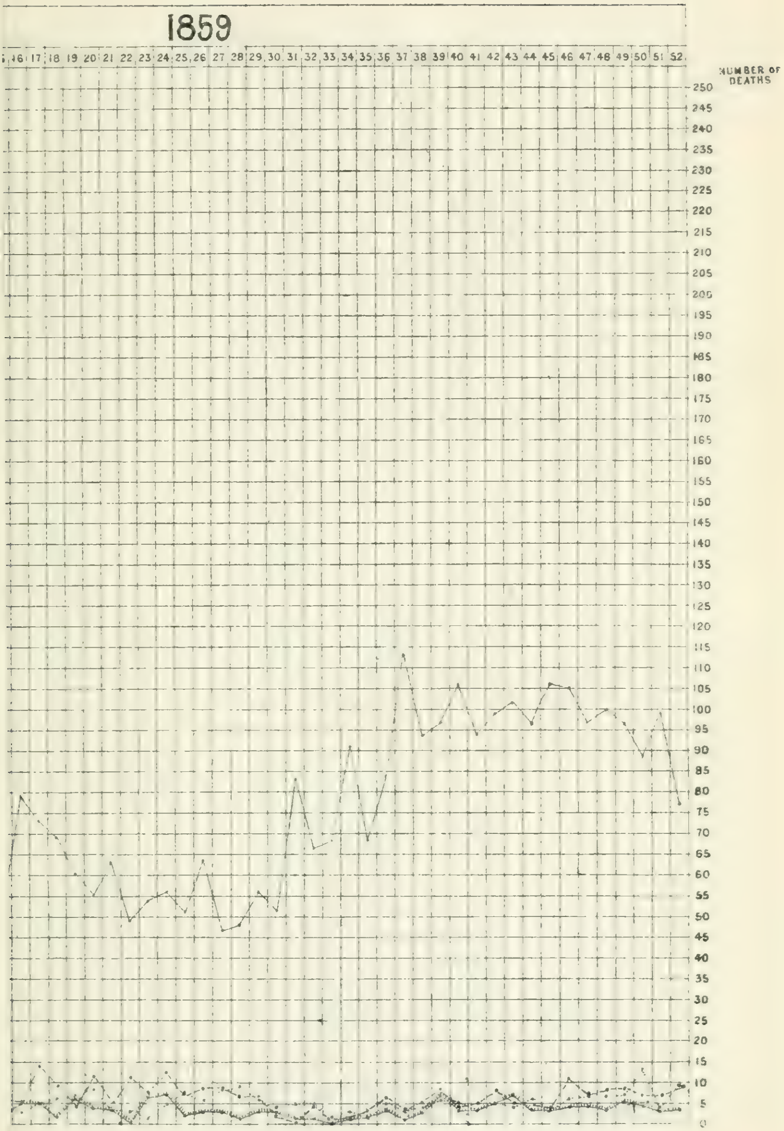
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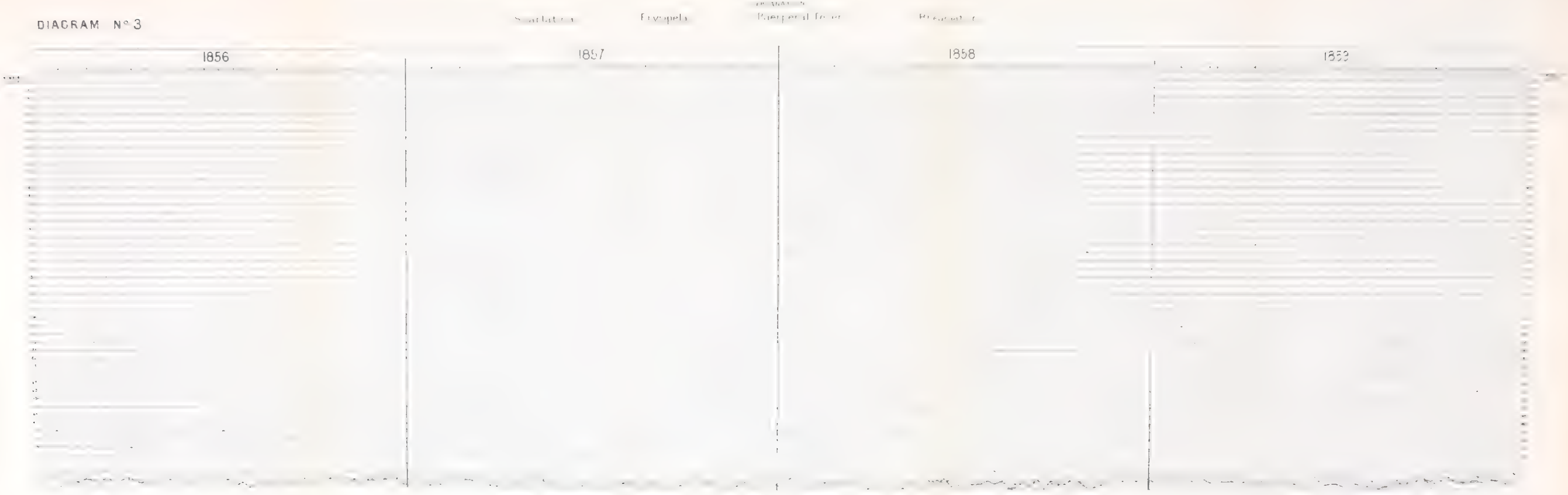
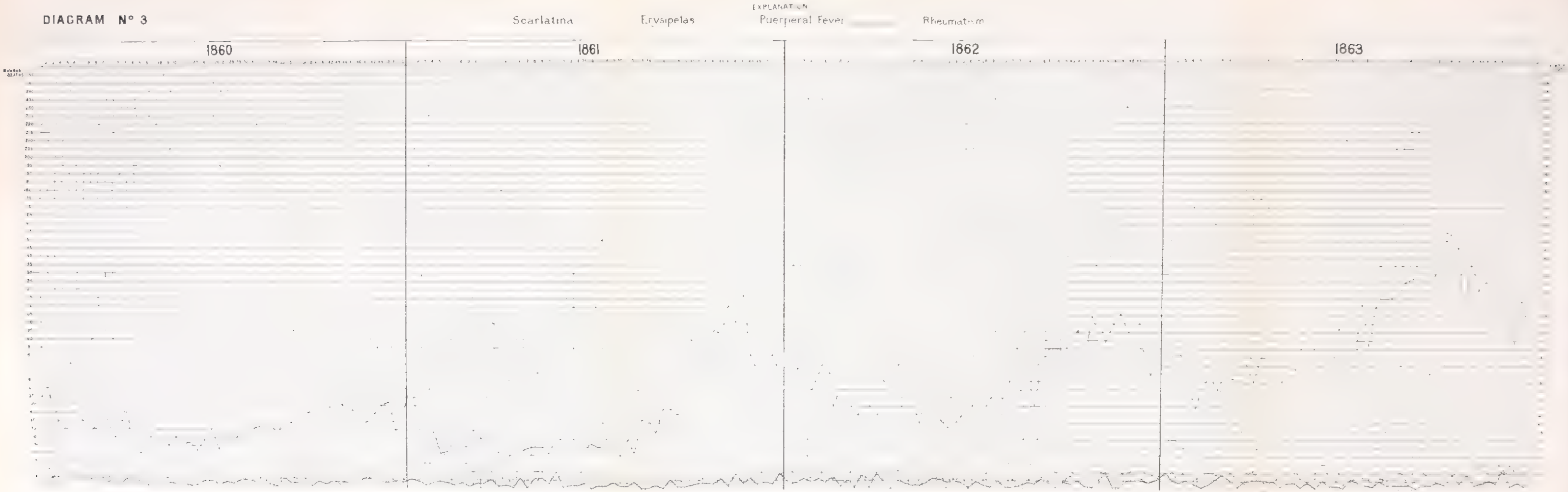
DEATHS FROM SCARLATINA PUERPERAL FEVER ERYSIPELAS & RHEUMATISM IN EACH WEEK OF THE YEARS 1848 TO 1874 INCLUSIVE

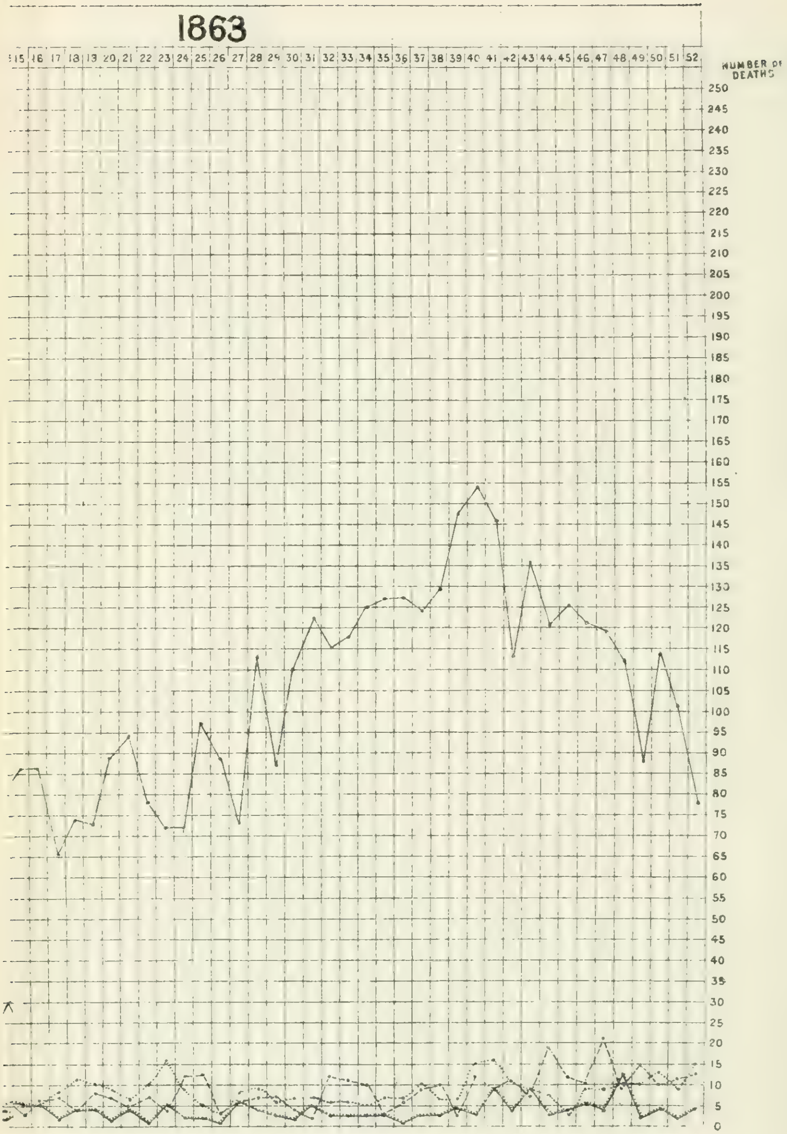
DIAGRAM Nº 3



DEATHS FROM SCARLATINA PUERPERAL FEVER, ERYSIPELAS & RHEUMATISM IN EACH WEEK OF THE YEARS 1848 TO 1874 INCLUSIVE

DIAGRAM N° 3

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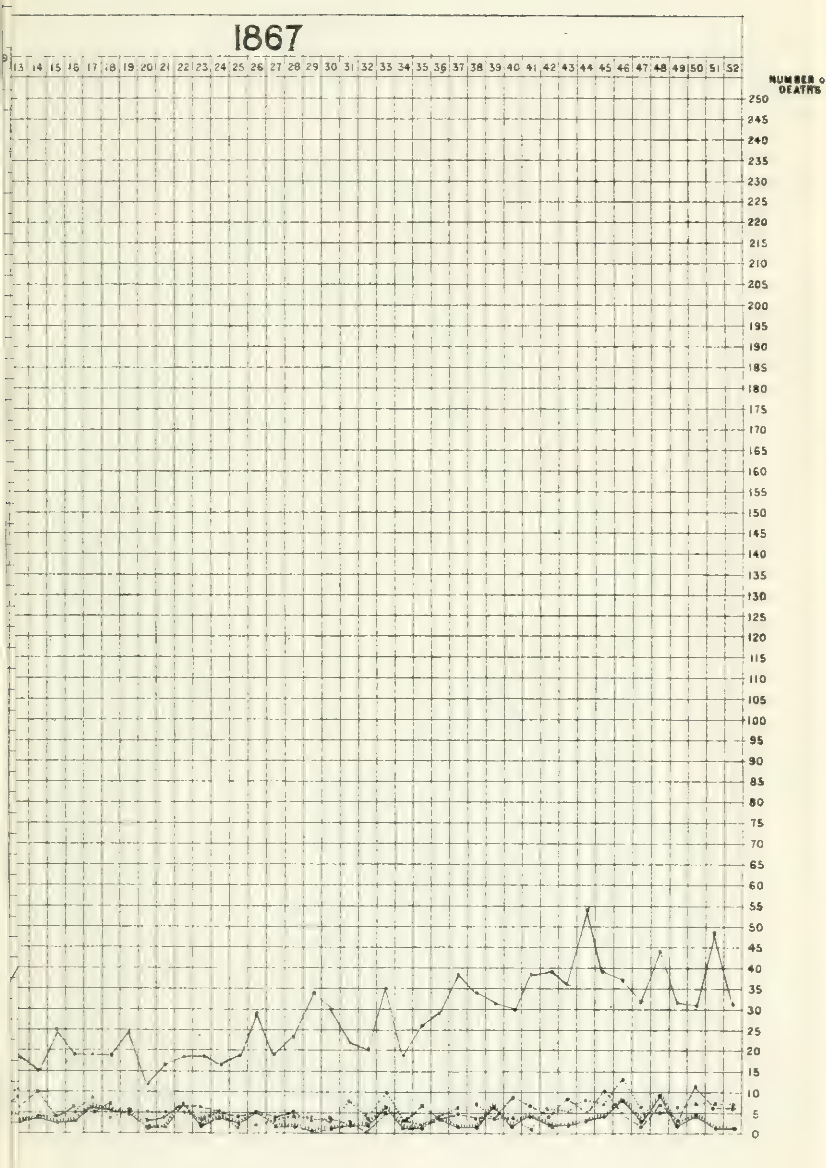
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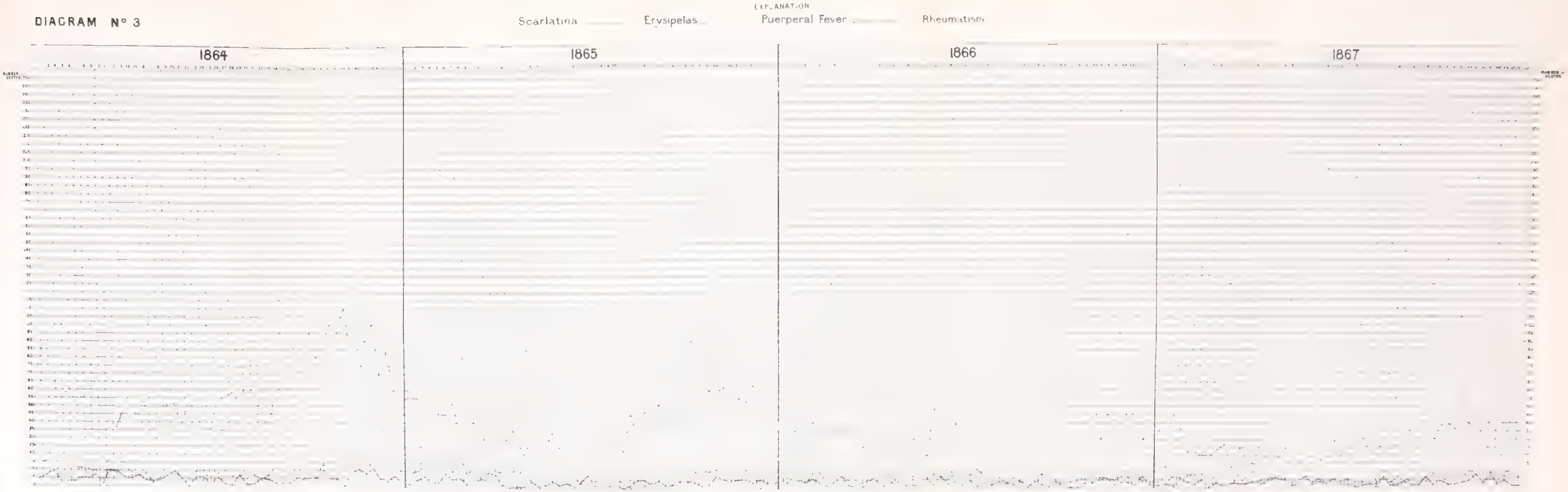
DEATHS FROM SCARLATINA PUERPERAL FEVER, ERYSIPELAS, & RHEUMATISM IN EACH WEEK OF THE YEARS 1848 TO 1874 INCLUSIVE

DIAGRAM No 3



DEATHS FROM SCARLATINA PUERPERAL FEVER, ERYSIPELAS, & RHEUMATISM IN EACH WEEK OF THE YEARS 1848 TO 1874 INCLUSIVE.

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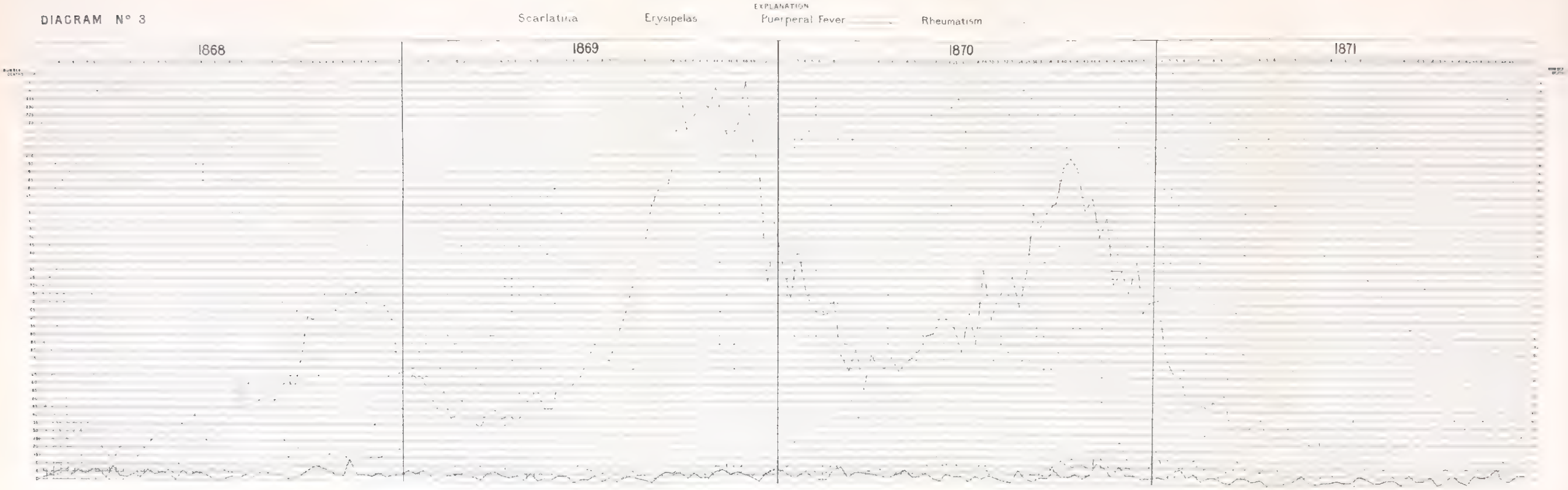

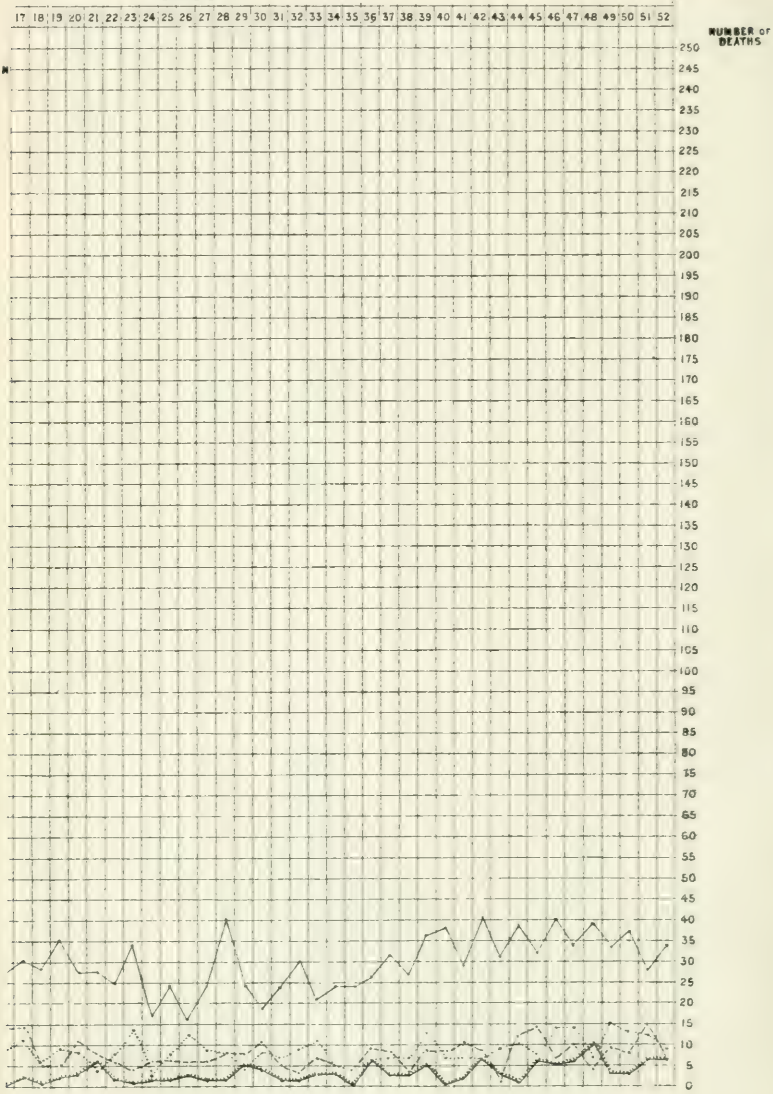

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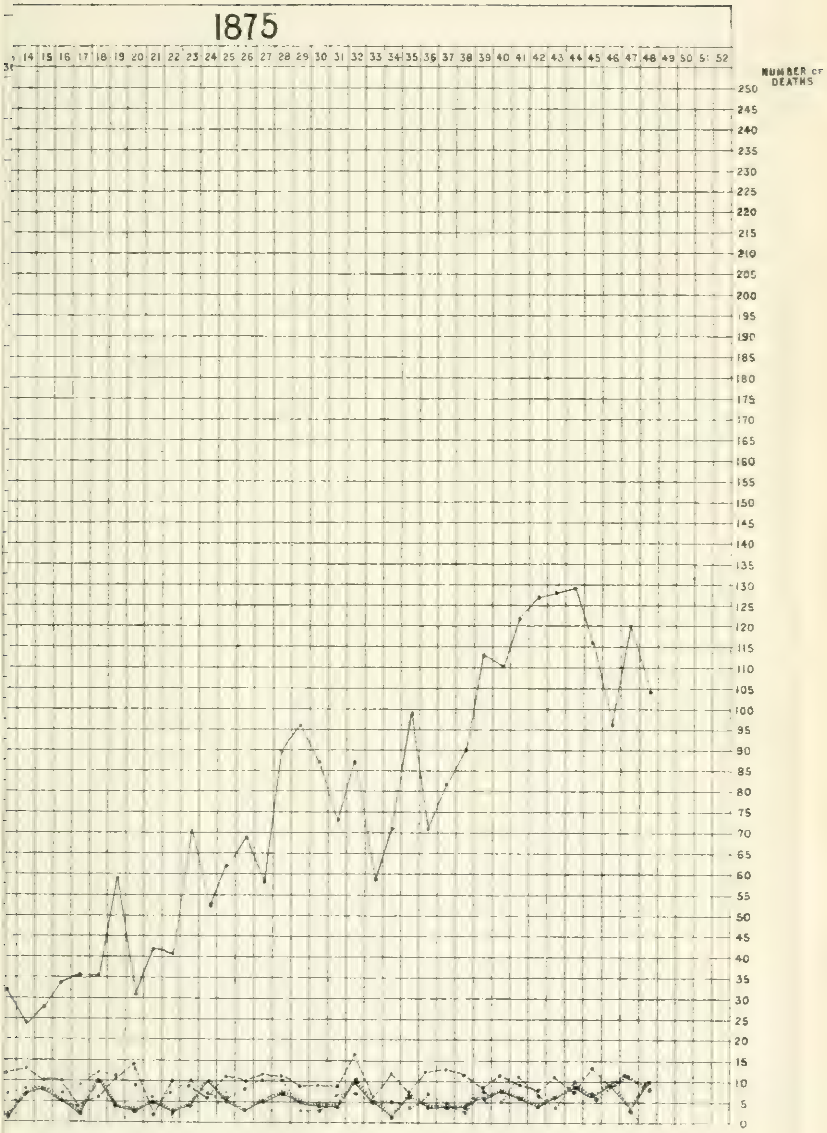
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DEATHS FROM SCARLATINA PUERPERAL FEVER, ERYSIPELAS, & RHEUMATISM IN EACH WEEK OF THE YEARS 1848 TO 1874 INCLUSIVE.

DIAGRAM Nº 3

EXPLANATION

Scarlatina Erysipelas Puerperal Fever Rheumatism

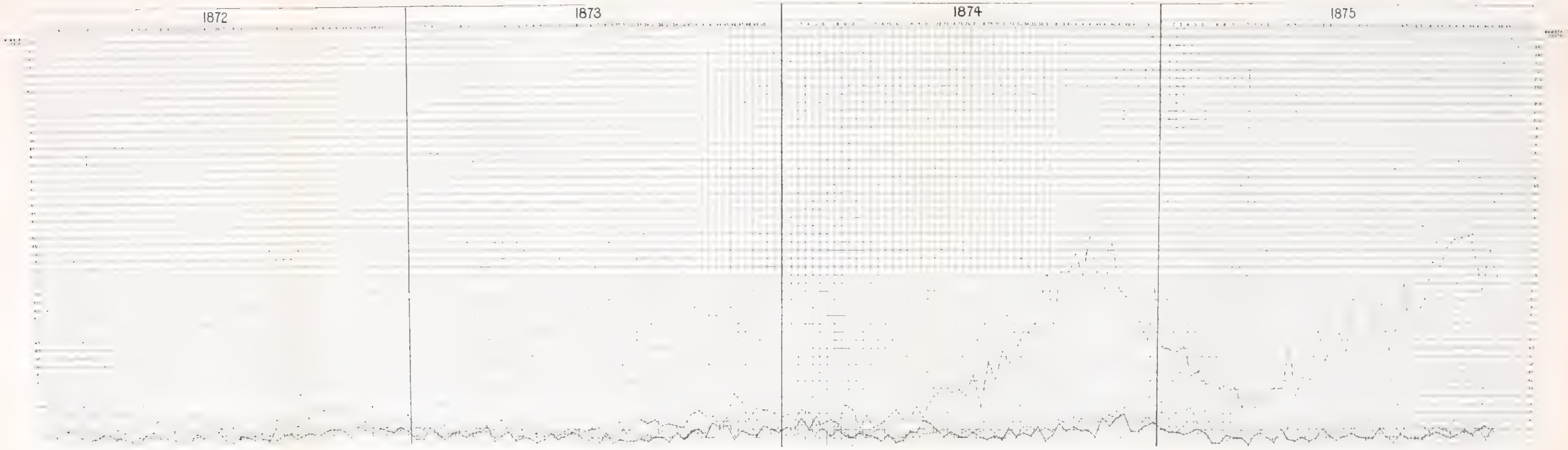

TABLE II.

Showing Highest and Lowest Weekly Mortalities.

	Lowest.	Highest.	Proportion.
Scarlatina	3	245	1 to 81
Rheumatism	0	27	0 to 27
Erysipelas	0	26	0 to 26
Puerperal Fever	0	19	0 to 19

This Table II. demands a few observations.

The evidence it affords is very incomplete, and would be misleading were it not corrected by a study of the full evidence in Diagram III.

The epidemic disease, scarlatina, rises from a lowest mortality of 3 per week in non-epidemic times, to a highest mortality of 245 per week in a great epidemic, or a rise of 81 times its lowest non-epidemic amount.

The rise of puerperal-fever mortality from an occasional weekly zero to a rare maximum of 19 weekly, is no evidence of epidemic character, for much greater rises and falls occur in diseases which are recognised as not epidemic, and occur also in pure accidents, as may be easily illustrated in any great census return.[1]

The rise of puerperal fever weekly mortality from 0 to 19 certainly affords scope for a small or slight epidemic; but the non-epidemic character of the rise is demonstrated, apart from the force of the negative considerations just adduced, by the Diagram III., which shows purely accidental and irregular variation—no gradual rise to beyond the range of the average, no cyclical epidemic climax, no rapid fall, no period of continued non-prevalence; as in the epidemics of scarlatina, of smallpox, and of cholera.

The assertors of puerperal fever epidemics can find nothing proportional to their awful statements in a rise from 0 to 19 in a vast city like London. The very smallness of the figure 19 is a destructive criticism on their statements.

The mortality of childbed from puerperal fever must be here referred to. I shall not discuss it, nor even attempt to state it. It is necessary only to remark, that it is utterly unnecessary to call in an imaginary epidemic to account for the rare occurence of so many as 19 weekly deaths in a city having about 2000[1] weekly deliveries, a figure implying that many more thousands are liable to be attacked among those delivered before and after the week in which the epidemic may be imagined to begin.

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[1] I might have here derived assistance to my argument by introducing a statement of the inevitable variations in frequency of every occurrence which has a uniform average (*i.e.*, in the present case, which is unaffected by epidemics). The very basis upon which theories of average have been mathematically constructed, necessarily implies rather a uniformity of deviations from the average, than a persistence of the dead average itself. The variations of the non-epidemic diseases here considered are illustrations of this fact, while the variations of scarlatina, smallpox, and cholera, obviously indicate a quite different law, namely, the fluctuations about a capriciously varying average.—I am indebted to Professor Tait for this note.

I now assert that I have shown that puerperal fever and erysipelas do not occur as epidemics—that they are not the subjects of epidemic prevalence; and I cannot see how any ingenuous mind can evade the force of the demonstration.

There may be error in my demonstration, I not only admit, but now take care to point out. This possible error is not one which will much diminish the force of my argument. The error lies in the curious possibility that there has not been an epidemic in London since registration was commenced. I say there is a possibility of this curious coincidence of absence of an epidemic during the only time when it was possible accurately to verify its existence. But the admission of this impossibility implies a rarity of childbed and erysipelas epidemics which, so far as assertions of such epidemics go, it is absurd to admit. For such epidemics are believed and asserted to be common, far more common than epidemics of cholera or epidemics of smallpox.

Puerperal fever and erysipelas have never been shown to be epidemic. They have now been shown never to have been epidemic in London during the only period in which the demonstration of their epidemic character was possible.

In conclusion, I may say that, while I adhere to London as my source of evidence, I have looked elsewhere pretty widely, and I am satisfied that similar statistics of other large towns and countries would supply evidence to the same effect as London affords.

I have said little of erysipelas, because it was not the chief subject of my investigation, and because almost all I have said of puerperal fever is true of it. The epidemic character of erysipelas is as much disproved as that of childbed fever.

I do not give a percentage diagram of the weekly prevalence of scarlatina and puerperal fever, and the other diseases exhibited in Diagram III. In the case of the yearly prevalence, I give such a Diagram (II.), on account of the error of the impression produced by Diagram I., in consequence of the great difference of the numbers slain by the various diseases. But an examination of Diagram III. shows that this is not necessary for judging the weekly mortalities. It is seen that the diseases are so nearly alike in actual numbers, when not epidemic, as to render a percentage diagram useless. The proximity of the lines of the various diseases shows this plainly for all except scarlatina. This disease is generally epidemic, and high above its neighbours in the diagram, and this circumstance is part of the proof of its epidemic character; but when not epidemic it closely adjoins the others. It has been for long periods in the non-epidemic state; so long, indeed, as to justify the establishment of an average of its frequency when not epidemic; and in this state its numbers are very near those of puerperal fever and erysipelas. In 1871, 1872, 1873, it was generally not epidemic; and, in accordance with the usual progress of such diseases, this condition of what may be called collapse of its destructiveness followed on a period of great slaughter in 1869, 1870. From these considerations it is plain that the diagram gives a sufficiently nearly correct view of the various diseases, and most strikingly shows the epidemics of scarlatina, but not a trace of such character in the other diseases. The immense majority of those slain by scarlatina dies in epidemics. In their absence, the destructiveness of the disease is like that of puerperal fever and erysipelas, which never rise into epidemics. A percentage table would therefore rather mislead than direct aright, for the mean of scarlatina would be terribly displaced for purposes of comparison by the weight of the epidemics. Indeed, as already said, there is no true or single average for scarlatina, but an average which varies according as the disease is epidemic or not.

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[1] As an illustration of the kind of statements made by the best authors, I quote from Meigs, who is well known to have opposed the chief popular ideas of the contagiousness and other fever-characteristics of puerperal fever. "But," says he (*On the Nature, Signs, and Treatment of Childbed Fevers, etc.* By Charles D. Meigs, M.D., etc. Philadelphia: 1854. P. 115), "when the disorder becomes epidemical, seizing upon a considerable part, or all the women who happen to be brought to bed within its range in a certain village, or hospital, or country, then the mind becomes astonished and bewildered, and hastens to lay hold on any probable or possible rationale of events so distressing, that they not only render the practitioner himself unhappy, but excite a great unquietude throughout the largest communities. Childbed fever, becoming epidemical, may prevail so extensively as to influence almost all the women who are brought to bed under its reign; or the force of the cause may be so slight as to produce illness, or only here and there an unfrequent example, so that the epidemic cases may be very rife or not."

I cannot pass on without directing attention to the very strong evidence (not proof) which this Diagram III. gives against the often-alleged alliance between scarlatina and puerperal fever. I can trace no relation between the prevalence of the two diseases. Indeed, there is an utter absence of any relation, as can be seen by comparing them at the time of the great scarlatina epidemics, such as in 1869 and 1870. Then, all the combined accoucheurs of London did not increase the amount of puerperal fever among their patients by attending scarlatina and childbed cases simultaneously. As to harm done in individual cases, I make only this statement, that it must have been very rare, for most accoucheurs do attend cases of scarlatina. Had it not been very rare, puerperal fever would have risen in fatality during the great epidemics. Like gratifying remarks may be justly made as to the influence of cases of scarlatina in the households of women confined or soon to be confined.

In this paper the argument has been hitherto directed to show that puerperal fever does not behave like any recognised epidemic disease, and especially not like scarlatina, with which it has been alleged to have a close alliance. Having demonstrated this, I find it unnecessary to renew a lengthened argument to show that it does behave like diseases recognised as having no epidemic character. This indeed follows almost as a matter of course from the prior demonstration. The student has enough in Diagram III. to show the non-epidemic prevalence of puerperal fever during twenty-seven years in London. It has, indeed, variations such as rheumatism and other non-epidemic diseases have; and to one cause of these variations, the only known one, I shall presently make reference. The only disease of a recognised non-epidemic character that I have placed beside puerperal fever and erysipelas is rheumatism, selected on account of the similarity of its numbers. I could easily have chosen for this purpose diseases, such as bronchitis, showing variations in frequency much greater than those of puerperal fever and erysipelas, but I thought it prudent not to encumber the diagram.

Mere great variations in numerical frequency do not constitute epidemic character any more than slight variations, such as those of puerperal fever and erysipelas. If any one, however careless, throws an eye on my diagrams, he will at once see a method or order in the epidemic variations which is not seen in the variations of non-epidemic diseases, whether these variations be great or small.

It has long been a favourite opinion of pathologists and practitioners, that there is a close alliance between erysipelas and puerperal fever, especially as regards intimate nature and cause of prevalence. With respect to the intimate nature of the diseases, this is not the place to say anything. The identity of cause of prevalence has been traced almost exclusively in and around individual cases and in hospitals. To such important evidence, I here make only passing allusion. Minor[1] has, however, recently, from American statistics of one year, tried to show a connexion between the wide prevalence of the two diseases. His elaborate work has been received by great authorities as settling this important point in a positive manner. To my mind his facts offer no proof whatever of his thesis; and the absence of proof is found also in my similar, but much more extended and much more highly elaborated, evidence. Any one who goes over the weekly mortality from these two diseases will probably, as I have done, fail to find any relation whatever between the varying prevalence of them. The seasonal relation is, indeed, very close; but this is a quite other matter, and to be proved in a different way. To prove any mutual relation, such as the causal, the influence of season should be excluded, which probably acts on both alike, and which has not been shown or even suspected of acting on the one through the other.

[1] Erysipelas and Childbed Fever. Cincinnati, 1874: Robert Clarke and Co.

There is no ground that I can find for asserting that when scarlatina is widely prevalent, puerperal fever is so, or *vice versa.* Neither is there any ground for asserting such mutual relation between erysipelas and puerperal fever.

But most if not all diseases vary in frequency according to the season of the year. With reference to scarlatina, puerperal fever, and erysipelas, much has been long made out, in a vague way, on this subject; and it is generally described as the injurious influence of cold or of cold and inclement weather. The great paper, however, of Buchan and Arthur Mitchell has now reduced these views to a state of comparative exactness, which future researches on a more extended basis may complete, but will probably not correct. The jagged curves of the diagrams of these gentlemen may lose their irregular outline, but will probably remain substantially as now.

These demonstrations have a very important bearing on the question of the alliance of these diseases—scarlatina, puerperal fever, and erysipelas.

The variations of scarlatina according to season (Fig. 1) or weather are quite different from those of erysipelas (Fig. 2) and puerperal fever (Fig. 3), and this important point is additional

SEASON CURVE OF SCARLATINA.

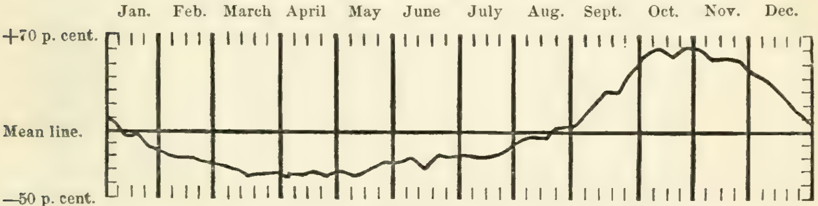

Fig. 1.

SEASON CURVE OF ERYSIPELAS.

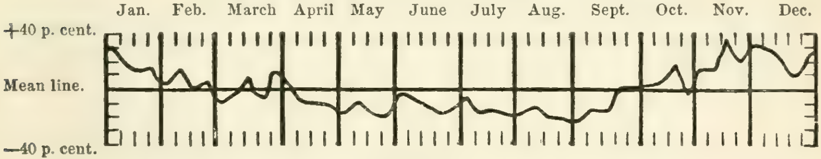

Fig. 2.

SEASON CURVE OF PUERPERAL FEVER.

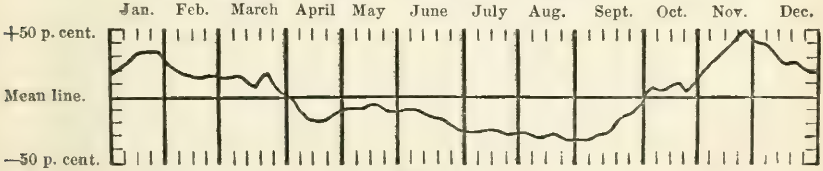

Fig. 3.

strong evidence in favour of the view, in this paper otherwise supported, that the diseases in their wide prevalence have no mutual connexion or influence.

On the other hand, the variations of erysipelas and puerperal fever, according to season or weather, are nearly identical, as Buchan and Mitchell have noticed; and this important point contributes to establish the probability of some alliance between these two diseases; an alliance which has received no support from the other facts adduced in the former parts of this paper.

In some diseases, as diarrhœa, the seasonal variation is so great as to justify in some degree, though not altogether, the application of the term epidemic to their regularly recurring enormous prevalence. In the diseases we have been discussing no such regular seasonal epidemic has even been alleged, and no trace of it exists. Increased prevalence caused by cold or other seasonal influence is not and has not been confounded with epidemic prevalence.

APPENDIX.

Diagram I. gives the yearly numbers or mortalities from 1848 to 1874 inclusive, all reduced to the population of 1874, to prevent the error that would arise from comparison of the numbers were this not done. Thus, there were in 1848, 652 deaths from cholera in a population of 2,244,837. To compare this mortality graphically with other years having different populations, we must reduce it proportionally, and all the other years likewise, to the same population; that, in the present case arbitrarily fixed on, of 1874. When this is done, we have, instead of 652 deaths in 2,244,837, 981 deaths in 3,400,701.

The lowest row of figures gives the years. The columns of figures on the sides give the numerical value of the mortalities. This diagram was made by Mr P. Scott Lang.

Diagram II. gives the percentages of the yearly mortalities above and below the average of 27 years from 1848 to 1874 inclusive.

The lowest row of figures gives the years. The columns of figures on the sides give the percentages above and below the zero or mean line for each of the diseases. This diagram was made by Mr P. Scott Lang.

Diagram III. gives the actual weekly deaths of the various diseases for the 27 years, 1848 to 1874 inclusive. The lowest row of figures gives the years. The columns of figures on the sides show the actual numbers of each succeeding parallel. This diagram was made by Mr P. Slater.

ARTICLE III.—*Case of Fibro-Cystic Tumour of Uterus; Removal of Uterus and Ovaries.* By THOMAS KEITH, M.D., Edinburgh.

EMILY C., aged 40, for more than five-and-twenty years a faithful nurse in an Irish family, came to me in February 1875 from Dr Millar of Londonderry, on account of a soft cystic uterine fibroid of rapid growth. Though the patient had been aware for more than a year that there was something wrong, Dr Millar's attention had been called to the existence of a tumour scarcely two months before. Since then it had increased much. There were profuse discharges of blood at the periods, and frequent attacks of pain. She had lost flesh, and was unable to discharge her duties.

The tumour extended three inches and a half above the umbilicus. Between it and the edges of the ribs on the left side one finger could be placed; on the right side two fingers. It was prominent, movable to some extent, one day elastic and hard, the next day soft and flaccid, so much so, that when seen in its state of relaxation, it might easily have been taken for an ovarian cystic growth. There was no bruit over it anywhere. The cervix uteri was not much enlarged. It lay far back, and unusually low in the pelvis. Movements of the tumour in any direction at once affected it.

There was, however, no doubt as to the diagnosis in this case; and Dr Millar had written that the patient, those interested in her, as well as himself, would agree to anything that gave her a chance of being restored to her former usefulness.

Till recently, I had looked upon the removal of fibrous tumours of the uterus by abdominal section as an unjustifiable operation under any circumstances. Had this patient come six months before, I would simply have sent her back again, lamenting my inability to give her any relief, leaving her to take the chance that, after eight or ten years of chronic invalidism—should she live so long—the tumour, on the cessation of the menses, would quiet down and give no farther trouble, unless what arose from its bulk. Of such a favourable termination there was here little hope; for the tumour was one of those soft cystic growths that often destroy life as quickly as ovarian tumours. Within the last few months (*Lancet*, 15th May 1875), I had operated in two cases of fibro-cystic disease of the uterus, in one case supposing the tumour to be ovarian—the only error in diagnosis in more than two hundred operations—in the other, almost driven into operating by the dogged determination of the patient to have her tumour taken away. Both of these were miserably constitutioned women, worn out and emaciated, bad subjects for the simplest operation: yet both recovered, and are now well. Dr Millar's patient could not thus now be summarily disposed of in the old way and sent home. After anxious thought, the removal of the uterus with the ovaries was advised and agreed to.

This was done on the 15th of February last, with the assistance of Dr Sidey and Dr M'Gibbon. Sulphuric ether was given by Dr Foulis. Dr Keith, Dr Dixon, Dr Wolston, and other visitors, were present. An opening was made ten inches in length, and the tumour was slowly pressed out of the abdomen. Both ovaries came with it, well up on the tumour. The broad ligaments were very large, covered with great veins, and full of cysts between the layers on either side. Each broad ligament was secured separately below the cysts; then a strong steel-wire was passed round the thick neck of the tumour, immediately above the vagina, and firmly secured by Koeberlé's instrument. The uterus, ovaries, tubes, and masses of cysts in the broad ligaments were then cut away. As, at this stage, it seemed doubtful whether the cut cervix could be secured external to the peritoneum without too much tension on the vagina, a piece of thin indiarubber cloth was wrapped round the neck, and fastened by another serre-nœud, embracing also the two tied ends of the broad ligaments. A sort of tube was thus formed, separating the strangulated tissues from contact with the living. The intention was to let the whole thus protected drop back into the pelvis, leaving the indiarubber funnel to give exit to any discharges. It was found, however, just possible to secure the stump in the lower angle of the wound, though with no small tension on the posterior wall of the vagina, by passing two soft needles through it, and bending them at a considerable angle. Several bleeding points were secured by Lister's ligatures, and care was taken to prevent blood from getting into the peritoneal cavity. The omentum was drawn downwards, and spread over the intestines as far as possible into the pelvis. The wound was closed by silk and horse-hair sutures. The only dressing used was some folds of antiseptic gauze, covered by cotton-wool and a bandage.

The weight of the tumour some hours after its removal was 8 lb. 4 oz., but a large amount of serum had already oozed from it. This did not coagulate on exposure to the air. The uterine cavity was three inches in length, in addition to about an inch and a half included in the part secured in the wound. In neither of my former cases was the cavity enlarged.

The operation lasted an hour. The anæsthesia with sulphuric ether was quiet and perfect. There was no vomiting during the operation or after it. In the afternoon, two opiate enemas (20 and 15 minims of laudanum) were given to quiet pain. By evening, there was moderate perspiration, also some refreshing sleep. A few turns of the screw were necessary to check some oozing from the stump, the surface of which was the size of a five-shillings piece.

Second day.—Good night with opiate, flatus coming up freely, much perspiration; urine only six ounces, loaded with lithates; pulse 80, temp. 100·7°.

Third day.—Troubled with pain and flatulence till afternoon, when flatus passed downwards, fifty-two hours after operation. Removed large serre-nœud, leaving the wires; pulse 70, temp. 100·7°.

Fourth day.—Restless night from dreams; pulse 66, temp. 100°.

Fifth day.—Good deal of pain round umbilicus; fœtid discharge round pedicle.

Sixth day.—Abdominal pain severe; general tympanism; pulse 84, temp. 100·6°.

Seventh day.—Abdomen full and tender; flatus does not pass well; pain sometimes severe; pulse 90, temp. 100°.

Ninth day.—Bowels acted freely with oil. She looked and felt well—all anxiety seemed to be over. An hour and a half after action of the bowels, sudden hæmorrhage came on from the wound. It was detected at once, and I was at her bedside a few minutes after the alarm was given. On removing the bandage, the large pad of cotton-wool was soaked in blood up to the sternum, and blood was flowing in a stream over the pubes, forming a pile of clot between the thighs. It was most alarming. Fortunately the patient was calm, the apparent coolness of the nurse preventing her from taking fright. The blood came from below and appeared at the surface, at the upper edge of the now hardened slough. Pressure and perchloride of iron were trusted to. For half an hour the bleeding broke out again and again in a most determined way, but by stuffing all round the slough with lint soaked in perchloride of iron, and making pressure with the fingers, it was at last checked, though the hand was not removed for a couple of hours, when a compress secured by plaster took the place of the fingers. The patient looked exhausted, for the pulse had become rapid and feeble. A large opiate was given, and repeated at night.

Tenth day.—Slept most of the night, and remained fairly quiet till the afternoon, when she became restless from pain in the back. As the hard crust made by the iron confined the discharge, it was partly cut away, giving outlet to some very putrid fluid. This relieved the back pain, and she had a good night with an opiate; pulse, 115; temperature, 101·5°.

For the next five days she looked ill, and lost flesh rapidly. On the fifteenth day the slough came away in a mass with the wires, needles, and indiarubber protective. A deep cavity was left, into which the finger passed three inches. Upwards of an ounce of intensely putrid pus had collected behind the slough, which was found to have extended nearly an inch beyond the ligatures.

A rapid convalescence followed, and she returned to Londonderry, quite alone, thirty-two days after operation. She at once resumed her duties as nurse, taking charge of a heavy child; and though she wrote some months afterwards that she was stronger than she had been for years, she has since had an attack of acute rheumatism, from which she is now convalescent.

ARTICLE IV.—*On Inflammation.* By G. THIN, M.D., London.

(*Continued from page* 694.)

CHAPTER IV.

EXTRAVASATION OF THE BLOOD-CORPUSCLES.

I HAVE in the two preceding chapters described the appearances which are to be observed in the cornea after it has been inflamed by irritants; and it has been shown that, although inflammatory changes take place in a non-vascular tissue, the intimate dependence of such an inflamed tissue on the nearest bloodvessels is soon demonstrated by the presence in it of the formed elements of the blood. The difference between a non-vascular tissue like the cornea, and the vascular connective tissue of other organs, is after all one of degree and not of kind. As regards its nutrition and also as regards the changes that take place in inflammation, the cornea is analogous to the islands of tissue, which are encircled by capillary bloodvessels, without being in actual contact with them.

The discovery that in inflammation the colourless corpuscles of the blood pass from the interior of the vessels into the surrounding tissues, and that they then constitute the formed elements seen in pus, and termed pus corpuscles, marks a new era in pathological science. The history of the discovery presents features of peculiar interest.

As early as 1843, Addison[1] maintained that the colourless corpuscles of the blood, mucous, and pus corpuscles, are identical structures, founding his belief upon their presenting the same optical characters and behaving similarily when brought in contact with different chemical agents. He further maintained that in the inflamed web of the frog's foot, the colourless corpuscles could be seen in the channels in which the red blood-corpuscles were visible, in the walls that bound these channels, and in the adjacent tissues. His views were not accepted by his contemporaries, who believed that the capillary vessels were closed tubes, out of which the corpuscles could not pass. Addison, not admitting that the capillary vessels were tubes with independent walls, found no such difficulty, and held firm to the facts which he satisfied himself could be observed. His description is clear enough :—"The walls bounding the capillary currents become much more evident and more strongly defined in the inflamed vessels; they appear, however, to consist of parallel fibrinous fibres, which gradually amalgamate with the structure of the tissue, as they recede from the current of the blood; and it is among these fibrinous-looking fibres that the lymph globules are seen to accumulate."

To Addison must be assigned the merit of having first identified the colourless corpuscles lying free in the tissues with the similar corpuscles seen in the bloodvessels.

The belief then prevalent amongst physiologists, that cells such as those seen by Addison are formed *de novo* prevented his views being accepted.

In the *Philosophical Magazine* for 1846, Dr Augustus Waller, then a young medical practitioner in Kensington, published two papers, in the first of which he showed how the tongue of the frog can be made available for the study of the tissues in the living animal; and in the second, how in the inflamed tongue the colourless blood-corpuscles can be seen to penetrate the walls of the capillaries, and, passing into the tissues, there become "pus-globules."

Having secured the frog's tongue over a hole cut in a slip of sheet cork, and allowed it to be exposed to the air for two or three hours, he observed that over the "whole surface of the tongue there were numerous colourless corpuscles outside of the vessels,

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[1] Experimental and Practical Researches on Inflammation, etc.

and occasionally a few scattered discs (red corpuscles). . . . In some instances, the manner in which the corpuscle escaped from the interior of the tube could be distinctly followed; that part of the tube in contact with the external side of the corpuscle gradually disappeared, and at nearly the same time might be seen the formation of a distinct line of demarcation between the inner segment of the corpuscle and the fluid parts of the blood in contact with it. Any slight agitation then was capable of disengaging the corpuscle from the vessel to which it was now external, and in its place a concave depression remained which appeared sufficiently protected by some membrane as to oppose effectually the exit of the discs and the fluid parts of the blood."

Stricker, in the Introduction to the *Studien*,[1] quotes the above passage and claims for Waller the priority of the discovery, but at the same time expresses the opinion that neither the text nor the figures in the plate which accompanied it were calculated to awake confidence. He rests this opinion on the fact, that Waller neither described nor figured a "neck" or pedicle in the corpuscle which was in the act of passing through the capillary wall.

I cannot agree with Stricker, in believing that the cause of Waller's great discovery being allowed to fall dead, was any omission in the description. Waller described appearances that were capable of verification, and he told his readers how to verify them. All that was required was a frog, a sheet of cork, a microscope, and a moderate amount of patience. The problem to be solved was of importance enough, it might be supposed, to have overcome the natural suspicion with which statements of new facts are always regarded, and to have provoked a repetition of Waller's experiment. But this does not seem to have been the case. I can find no evidence that any of the distinguished physiologists contemporary with Waller thought it necessary to follow his simple directions, or to take any notice of his papers whatever. Had they done so, the "neck" of the corpuscle would have been observed in due time. As it was, one of the few great discoveries that have marked a turning point in the history of medicine—and not the least of them—was locked up in the pages of the *Philosophical Magazine* for twenty years.

It was believed at that time by the best authorities that "pus cells" are formed in a blastema; and so impregnable was this theory, that Waller's description of how their true origin might be seen was disregarded on account of its inherent improbability. It is curious to note that although the faith in the blastema theory was too strong for a demonstrable truth, it could not resist another hypothesis, the "proliferation of the connective-tissue corpuscles," which the genius of Virchow imposed unconditionally on Europe, and which was received for twenty years with scarcely a protesting voice.

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[1] Loc. cit.

Augustus Waller, the first observer of the emigration of the colourless corpuscles of the blood in inflamed tissues, after a life spent in alternate intervals of medical practice and physiological investigation, finally settled at Geneva in 1868, and died there in 1870.

Cohnheim, who knew nothing of Waller's papers, again discovered the emigration of the colourless corpuscles in the inflamed mesentery of the frog, and published his discovery in *Virchow's Archiv* in 1867. Thanks to the high scientific reputation of the author, and the medium through which his essay was communicated to the scientific world, it at once attracted the attention of pathologists. His experiments were immediately repeated; and although some observers were not able at first to follow all the details of the process as he described them, the accuracy of his observations has within the last few years been abundantly attested.

Experiments of the kind had in the meanwhile been much facilitated by Stricker's discovery, that diapedesis of the red corpuscles can be observed in frogs which are under the influence of curari.

When inflammation is produced in a vascular organ, there are certain phenomena which are always present, and which must therefore be considered as essential parts of the inflammatory process. An account of these, as they are seen in a transparent tissue, will serve as a typical description; and in the interest of the reader, I will, although conscious of my inability to do justice to the original text, translate Cohnheim's description of the appearances observable in the inflamed frog's tongue. I quote from a work he has lately published.[1] "When a small piece of nitrate of silver is placed on the surface of the tongue, slight muscular twitchings generally appear in the neighbourhood of the caustic, a rapid dilatation takes place first in the arteries which supply the cauterized spot, and then in the veins which have their origin in it. All the capillaries of this part are filled with blood in the most striking way. The hyperæmia extends not only to the vessels whose branches are directly acted on by the caustic, but also (especially if a somewhat large piece has been used) always to other vessels in the neighbourhood, so that the hyperæmic zone may attain very considerable dimensions. As long as the dilatation is increasing, and for some time longer, the blood flows in the whole of the widened vessels with great rapidity; and it is an exceedingly striking thing to compare the vessels of the hyperæmic zone with those which have not undergone dilatation, and in which, therefore, the blood flows at the original rate of speed. But the scene soon changes. First in the vessels whose terminal branches are directly in the cauterized spot, there are effects visible which show that in these branches the circulation is arrested. It is exactly as if the vessels, as far as the caustic directly reaches and acts, were tied, and rendered impermeable. In the arterial branch which leads directly to the cauterized part, the onward movement, after several futile and fitful efforts to drive the blood-column forward, comes to an end, and the blood in it stagnates; on the other side, in the venous branches which arise from the cauterized part, the movement becomes gradually slower and slower, until there also it comes to a standstill,—stagnation. In both arteries and veins, the stagnation extends to the first collateral branches which lead to and arise in parts beyond the cauterized spot. In these parts the circulation is still rapid, and all the more so in the arterial collateral branches, on account of the increased tension produced by the occlusion of the affected artery. In the veins, the arrest of the circulation in several of the radicles leads of course to the circulation being slower; nevertheless, even when the still open collateral radicles do not directly correspond to the collateral arteries, the general acceleration is usually at first so great, that in veins at whose origin stasis has taken place, the blood circulates more quickly than it did originally before the cauterization. Of course, there is stasis in a number of the capillaries, not only in all those which have been directly affected by the caustic, but also in some of those immediately adjoining, as far as they form the connecting channels between the stagnant arteries and veins. All these are the evident—so to say, the mechanical—effects of the caustic, and they exhibit manifold modifications according as a larger artery, or a vein of greater calibre, happens to have been affected. These modifications may be left without further consideration, as their varying features may be easily deduced theoretically, and as I have treated of them at large in previous publications.[1]

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[1] Neue Untersuchungen über die Entzündung. Berlin, 1873.

"To these changes in the condition of the vessels of the hyperæmic zone—which are of purely mechanical origin—are added, sooner or later, others which are not less striking. In about one or two hours after the application of the caustic, first the dilated arteries which are furthest removed from the cauterized part begin to contract, and therewith the circulation in them becomes slower. This is naturally followed by slower circulation in the corresponding veins, and the more so, as it is much longer before their dilated condition begins to diminish. The vessels which are nearer follow the more distant ones in this respect, until, after a few hours, in most of the previously dilated arteries, and a little later in all the corresponding veins, the lumen and the rate of circulation have returned to the normal condition. Only those vessels, arterial and venous, which lead directly into the cauterized part remain permanently dilated over a shorter or longer part of their course, and especially the veins, whose lumen remains permanently twice or three times the diameter of the veins which arise at a distance from the eschar, but which were originally of the same calibre as those affected by the caustic. The dilatation of the affected arteries is also usually considerable; yet the relation of the diameter between the respective arteries and veins, in accordance with which that of the latter is much the greater, is preserved. But even these vessels, although they have contracted so little, no longer present the same spectacle as formerly shortly after the application of the caustic. For, in spite of the dilatation, the circulation has become gradually slower in them, and not only in the veins, in which, in about five or six hours after the cauterization, the blood creeps along with a very lazy movement, but also in the arteries, in which there is no difficulty in distinguishing the individual corpuscles. This slow circulation extends often to the neighbouring branches, which had been for a considerable time the scene of a collateral accelerated circulation. The latter condition now shows itself in one or more of the still more distantly situated lateral branches.

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[1] Venöse Stauung. Virchow's Arch., Band 41, p. 220; and Embol. Processe, Absch. I.

"Whilst the condition of the arteries and veins in the vicinity of the cauterized part is such as has been described above, not less striking changes take place in that of the capillaries. Those more distant from the eschar have gradually, under the influence of the progressive contraction of the arteries, got quit of the excessive quantity of blood by which they were distended, and again present their original appearance of small channels, in which a light yellow stream flows with moderate rapidity, and in which the formed elements can be mostly recognised without difficulty. On the other hand, in the part immediately surrounding the eschar, the stasis in some of the capillaries remains complete; now, as formerly, are to be seen capillaries in which the contents have been moulded into an apparently homogeneous mass, whose colour is of a carmine to a bluish red, and in which there is naturally no visible trace of movement. The only difference, compared with their condition when stasis was beginning, is, that these small vessels seem wider. But what is much more striking is, that the number of stagnating capillaries has become larger; not only those which lie close on the eschar, but also a second and third zone, and even more, have passed into complete stasis, and in them also there is the same absolutely motionless homogeneous red mass. Further outwards from these stagnating capillaries, comes a broader or narrower zone, in which they are still more filled with blood than they originally were, and in which movement is still undoubtedly present, but is exceedingly slow; and it is only beyond these that we come on capillaries which, in their condition, can in no way be distinguished from those of the rest of the tongue. The picture which, as a whole, that region of the tongue presents, which some time previously has been cauterized with nitrate of silver, may be summarily described thus:—

"In the immediate vicinity of the eschar, which has in the meanwhile become black, there is a somewhat broad zone of complete stagnation in all the vessels ; further, the arteries and veins which lead directly into the eschar are dilated over a greater or less extent, and the blood flows through them slowly ; finally, a similar slow circulation and distension with blood is seen in a circular zone of capillaries which bounds the periphery of the region of perfect stasis ; everywhere else in the tongue the vessels present absolutely their usual condition. In most cases this is the picture which can be seen six or eight hours after the application of the caustic ; but I have, in the above description, intentionally abstained from giving any fixed data in figures, either as regards the duration of the separate stages or the relative calibres of the vessels, because the greater the number of frogs (rana esc.) on which I studied these appearances became, the more was I convinced that, in this respect, there are very great differences. Whilst, in one animal, the stage last described is reached in two to three hours, ten or twelve elapse before it is reached in another. As might have been *a priori* expected, the whole proceeds quicker in summer-frogs than in winter-frogs ; but that is the only difference between them.

" In the course of the next six or eight hours a new set of phenomena are to be observed in the vessels. First, there is often still a good deal of dilatation, especially in the arteries, less striking, although quite evident, in the veins, which are situated near the cauterized part, without, however, touching it ; a condition that, in the rich supply of vessels to the tongue, must often be the case. But what is most interesting in these dilatations is, that they are usually quite limited, being entirely restricted to that part of the vessel which is adjacent to the eschar ; whilst beyond this part, in both directions, the same vessels are of the usual width. In the sections of localized dilatation, the circulation is naturally very slow ; and, of course, most so when it occurs, as is often the case, in one of the lateral branches of the artery which leads to the cauterized part, in which, as follows from the explanation given above, the stream is in any case slow. Secondly, there commences at this time, with very small beginnings at first, a process which henceforth will more than all else engage our attention, namely, extravasation. Generally it is some of the capillaries in the zone of slow movement, but sometimes one of the dilated veins, whose contour is first rendered uneven by extravasating blood-corpuscles ; if it is a vein, then it is always colourless corpuscles, which, as I may remark in passing, accumulate regularly on the inner coat of all the veins at the time the circulation becomes slow, and remain permanently in this position ; from the capillaries, on the other hand, both coloured and colourless corpuscles pass out from the beginning. The process, which takes place to a slight degree at first and at isolated points, becomes gradually more and more general. The veins are surrounded by colourless corpuscles, which soon embrace them several rows deep, and not the less increases more and more the passage outwards of the blood-corpuscles from the capillaries. At first it may have been only the capillaries of the zone of slower circulation from which the extravasation took place; but after some time those of the external stasis-zone take part in the process. But here the diapedesis of the red corpuscles is greatly in excess. The capillaries become bordered with even thicker masses of them, so that they to a great extent fill the intermediate spaces between the adjoining bloodvessels. This does not extend to the inner stasis-zone; here the contour of the distended capillaries remains quite even;—not a single corpuscle is being extravasated. On the other hand, in the course of the next and the following days, the process extends further peripherally, and that in two ways. The belt of distended capillaries through which the blood has been hitherto slowly moving, now passes into more or less complete stasis, and becomes conspicuously the seat of an ever-increasing diapedesis; but further, the next zone, which has been hitherto free, begins to be affected, and the capillaries become fuller, the blood flows through them more slowly, and there is an emigration from them of white and red corpuscles. The breadth of this last zone may be somewhat extensive. Meanwhile the emigration from the affected veins continues, and in the smaller veins, a few red corpuscles pass out with the white, which constitute by far the greatest proportion; only the contour of the arteries remains perfectly even. At the points of localized dilatation, where the circulation is very slow, and where mostly the arrangement of the white corpuscles along the inner wall of the vessel is exceedingly marked, not a single corpuscle leaves the channel; contrasting so much the more with the localized dilatation of the veins, where an abundant contingent is given to the extravasation. Accordingly, on the second, third, and fourth day after the cauterization the appearances are the following:—Around the eschar a small zone of absolute stasis, then a relatively broad zone of stagnating capillaries, from which an enormous number of red corpuscles pass outwards, then a still broader zone of capillaries with a slow circulation, from which the most abundant extravasation of coloured and colourless corpuscles takes place; at the same time considerable extravasation from the dilated veins in the neighbourhood of the eschar, in which the blood is moving slowly. At some distance from the part everything is normal. The capillaries adjoining the zone of extravasation are fuller than is the case in the ordinary conditions; but of extravasation of red or colourless corpuscles nothing is to be observed, and still less in the capillaries further removed, in which the contents and current are normal. But the extravasation from the veins is limited to the vicinity of the cauterized part; at some distance there is nothing of it to be seen, let the current be ever so slow, the dilatation ever so considerable. In the veins which take their origin directly in the cauterized spot this remarkable condition is often enough observable; as has been already described, they are regularly dilated and the blood flows in them with great slowness until the point where they receive another branch, the circulation in which, being normal, produces more active movement in the venous trunk. Invariably, as far as the slow circulation extends, there is the most typical accumulation of colourless corpuscles on the inner vascular wall, but extravasation is seen only in the vicinity of the eschar; it ceases completely at some distance from it. Thus the eschar is now surrounded by a crowded mass of red and colourless blood-corpuscles, the former, on account of their colour, being naturally the more conspicuous, but by careful observation the colourless cells can be recognised everywhere amongst the red ones, and in the peripheral zones they outnumber by far the coloured ones; on the other hand, the former are often seen close to the eschar, in the area of complete stasis, whither they have in such cases wandered from without. There is, further, at this time, always a great number of colourless corpuscles in the corresponding lymph-spaces of the tongue, especially in the principal sinus which is situated in the posterior attached portion. But that it is not only the formed elements of the blood which have found their way from the circumference of the eschar into the lymph-spaces of tongue, is shown by the considerable and often tense fulness of the lymph-sacks with watery fluid. This comes from the vessels round the eschar, being a product of the active transudation which has accompanied the extravasation of the corpuscles of the blood. To this is also to be referred the fact, that the eschar projects above the level of the rest of the tongue. This is always the case, and often to a considerable extent, when the cauterization has not affected the whole thickness. If it has done so, only the borders of the eschar are elevated, the centre constituting a pit-shaped depression. Everything remains for a series of days in the condition described; but extravasation and transudation finally cease, the lymph-sinus at the root of the tongue collapses, the colourless cells gradually disappear from the circumference of the eschar, whilst the red ones remaining form the bloody circle which is long seen around it; but weeks usually elapse before the black eschar is completely loosened and removed."

The principal features in this account of the changes which constitute inflammation are divisible into two categories. The first in order of time consists in dilatation of the arteries and veins, consequent distension and rapid circulation in all the vessels of the part, and accumulation of colourless corpuscles on the inner wall of the veins.[1]

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[1] A momentary contraction of the arteries has been observed by Mr Lister and others to follow immediately the application of some irritants. It gives place in a few seconds to dilatation, and it is evidently not an essential link in the chain of phenomena. I do not enter on the question of the mechanism

Cohnheim has proved by a series of experiments, that it is possible to produce these effects without the process proceeding further, the circulation gradually becoming normal, not a single corpuscle having left the vessels ; up to this point he does not believe that the term inflammation is applicable. But when these changes have been produced by the application of a caustic, they are followed by a second series, of which permanent dilation of the artery and vein leading to the cauterized spot, a progressive stasis in the capillaries, and extravasation of blood corpuscles are the chief. These facts have received explanation on several hypotheses. That given by Cohnheim I quote in his own words. "By the application of the caustic, a sudden injury to the vessels and disturbance of the circulation of the nutritive fluid in the tissues is produced, the result of which is a dilatation of the vessels of the part without the co-operation of any reflex action. The accelerated circulation which necessarily accompanies the dilatation begins after a time to slacken, first in the arteries, later in the veins, so that the effects which follow the application gradually subside. Then there begin slowly, and step by step, the changes which are produced on the vessels next the eschar by the direct action of the caustic and of the chemical products of which it has been the cause. These gradually extend outwards, but always so that the vessels nearest the eschar are most, and those further removed least affected." Cohnheim believes that the sole cause of the stagnation and the extravasation is to be found in an abnormal condition of the walls of the vessels directly produced by the caustic. He has shown by experiments on the nictitating membrane of the frog and the rabbit's ear, that stasis and extravasation can be observed without the preliminary dilatation of the vessels, if care is taken to apply the caustic, so that no vessel is directly touched by it. He considers that he has eliminated the possibility of reflex action by showing that the phenomena take place in the same order in the frog's tongue when all connexion between the tongue and the animal has been severed, except the lingual artery and vein. To the exception that might be taken to this experiment, namely, that a reflex action might be possible through the medium of the vaso-motor nerves, he points out that no ganglia have been shown to exist on these nerves.

Mr Lister has shown, many years ago,[1] by a series of simple and well-devised experiments, that at least one cause of the tendency of the corpuscles in the blood which flows through an inflamed

by which the muscular fibres of the arteries suddenly relax, because I do not feel warranted in discussing an unsettled and difficult physiological problem, to the solution of which I have nothing to contribute. The view which considers it as being nothing more than paralysis of the muscular coat of the arteries, and probably also of the veins, as the direct effect of a local cause, seems to me, however, in the absence of direct proof to the contrary, the more probable one.

[1] Phil. Transactions, 1858.

part to adhere to each other, and to the walls of the vessels, is connected with changes in the vascular wall, the vessels becoming so altered, that they act like a foreign body on the blood which is in contact with them. To illustrate the nature of the experiments by which this was shown, I quote part of the account given of one of them. "In a case," he remarks, "in which the circulation was perfectly natural in the web, and the corpuscles moving on at slight intervals, with no tendency to adhere, on a drop of chloroform being applied, I saw the very same corpuscles instantly become checked in their progress by sticking to each other and to the capillary walls, and move on slowly in masses, with considerable intervals. . . . That the effect on the blood within the vessels of a part inflamed by chloroform is secondary to a change in the tissues, is further proved by the circumstance that abnormal accumulation of slowly-moving corpuscles may last for hours together without stagnation, as a consequence of the application of this irritant for an extremely brief period. Long after all the blood which could possibly have been directly acted on by the chloroform has left the vessels, successive fresh portions continue to experience precisely similar changes in passing through the irritated area."

That the adhesiveness of the colourless corpuscles is not the only cause of their attaching themselves to the wall of the vessel has been shown by Cohnheim, who found that when the blood current was shut off by temporary occlusion of the main artery, the corpuscles left the wall of the vessel and mixed with the others in the centre of the vessel, but that when the current was re-established they again adhered to the wall.

Arnold has described minutely appearances presented by the colourless corpuscles, which indicate that they are acted on by the stream of serum which passes outwards through the weakened wall of the vessel, and assigns to this action an important part in the phenomena of adhesion and emigration.

As soon as the fact of the extravasation of the blood-corpuscles was established, the problem that presented itself for solution was how to account for the passage of solid bodies through an apparently unlacerated tube. The question received various answers, to which it is not necessary to refer, as the investigations of Professor Arnold and Dr Laidlaw Purves, carried on independently of each other, have solved the chief difficulty. The epithelium that constitutes the inner surface of the capillaries and smaller veins was first demonstrated by the injection into the vessels of a solution of nitrate of silver mixed with gelatine. After exposure to the light the outlines of the cells were seen as irregular black lines. By a similar procedure the points of exit of the corpuscles were determined. An animal in which the extravasation was going on was killed, and the vessels injected with silver solution. The colourless corpuscles were found fixed in the wall of the vessel, and it was then evident that they passed outwards between the epithelial cells. They were fixed in the dark lines that indicate the cell contours.

When epithelial membranes are treated by silver, a dark mass is often seen at the angles of junction of the cells. Great importance has been assigned by Arnold to such masses of albuminate of silver when seen in inflamed capillaries, and he describes them minutely, the smaller as stigmata and the larger as stomata. Exudation is supposed to take place freely at these pores; but the extravasation of the corpuscles is not limited to these points. Dr Purves describes the emigration as taking place " not seldom where three cells meet, but more often between two cells, and especially in the neighbourhood of the end of the cell."

The œdema that takes place so rapidly in inflammation, shows that there is an abundant passage of the fluid elements of the blood through the vascular wall, and the observations of Arnold show that this takes place chiefly, if not entirely, through the same interepithelial spaces in which the corpuscles can be seen. He infers this from observing the passive movements of corpuscles which are in the act of passing through. These justify the inference that there is a stream which passes through with the corpuscle, and carries it away from the point of exit.

That the vascular wall has, as a direct result of the inflammation, undergone a change by which it offers less resistance to the pressure of the blood, has been proved experimentally by Winiwarter.[1] He found that in the frog injection masses were extravasated from the vessels of the mesentery by a much weaker pressure when the mesentery had been inflamed than when it was healthy.

I have previously shown, that when a part of the cornea is cauterized the resulting change in the tissue can often be traced in a localized tract which stretches towards the conjunctival border. There the affected tissue is in direct contact with the walls of the bloodvessels, which in their turn suffer from the injurious influences which originate in the wounded part. As soon as the vessels have been reached, the usual phenomena of inflammation present themselves, and many of the extravasated colourless corpuscles find their way, as we have seen, into the corneal tissue.

The direct result of the inflammatory process, whether in a vascular or non-vascular tissue, is thus to fill the interstices of the inflamed part with the formed and unformed elements of the blood. These exuded elements provide the means of repair to the damaged tissues. The changes by which this is accomplished will be discussed in the next and concluding chapter.

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[1] Der Widerstand der Gefässwände im normalen Zustande und während der Entzündung. Wien. Akad. Sitzb. lxviii. 1873.

ARTICLE V.—*Diphtheria: Typical Cases, in illustration of its Diagnosis, Prognosis, and Treatment; together with Remarks on the Significance of Albuminuria in that Disease.* By Sir JOHN ROSE CORMACK, K.B., F.R.S.E., M.D. Edin., M.D. Paris, Fellow of the Royal College of Physicians of London, Chevalier of the Legion of Honour, Physician to the Hertford British Hospital of Paris.

DIPHTHERIA is a specific, contagious, asthenic, general disease. It sometimes occurs as an epidemic; but is always present in a sporadic form in some districts of country and in some towns—in Paris, for example.

The leading characteristic of diphtheria is an exudation, on mucous surfaces, wounds, and cutaneous abrasions, of cacoplastic lymph, which becomes the basis of a tough, leathery, coherent false membrane, which may be met with on the mucous membrane of the pharynx, larynx, trachea, bronchial tubes, nares, eyelids, and vagina, as well as around wounds. This false membrane often forms on the edges of the tracheotomist's incision in cases of laryngo-tracheal diphtheria.

The pellicle of diphtheria is a leathery membrane, tough and difficult to tear. It is not the product of "inflammation" in the ordinary acceptation of that term. It has as its basis a peculiar fluid exuded from the surface on which it rests. This fluid, which may be described as cacoplastic lymph, agglutinates a profusion of epithelial cells. The membrane thus formed consists of compact tenacious layers, which give it a stratified appearance.

The pellicle of scarlatinous sore-throat, on the other hand, is pultaceous and easily torn. In cases of scarlatinous gangrene of the tonsils, the substance which simulates the false membrane of diphtheria will, on examination, be found to be sphacelated mucous membrane, with the addition of a pultaceous substance—a loose epithelial detritus. In a word, the important diagnostic distinction consists in the diphtheritic pellicle being a strong, tough membrane, and the scarlatinous simulation of it being a pultaceous, easily-torn stratum of detritus, or a portion of gangrenous mucous membrane.

The formation of the diphtheritic pellicle generally begins on one or both tonsils after two or three days of some precursory redness and swelling, which are often so slight as to be unobserved. The pellicle, in most cases, first shows itself as a white patch of exudation on one tonsil: from this starting-point, formation of false membrane proceeds—generally, but not always, in continuity—rapidly to both tonsils, the veil of the palate, the uvula, and the pharynx. This extension is accomplished in young children in from one to three or four days, and in adults less rapidly. From the pharynx, except in mild cases, the false membrane descends into the larynx. It is not so in scarlatina, the larynx being comparatively seldom affected in that disease. To quote the familiar clinical saying of Trousseau—"Scarlatina has no liking for the larynx."

Nevertheless, there exists in some cases a great difficulty in establishing an absolute diagnosis between diphtheria and scarlatina, in consequence of the unquestionable relationship which exists between the two diseases, notwithstanding their being pathologically distinct from each other. Diphtheria—as Graves and others have shown, and as Trousseau admitted in his later teaching—often follows so close in the wake of scarlatina as to seem to be one of its later stages. False membranes—diphtheritic membranes—form in the pharynx and larynx, as an immediate sequel of scarlatina. In such cases, diphtheria has closely followed or been engrafted on the original attack of scarlatina.

Discussions—unprofitable, meaningless discussions—have taken place in respect to the identity or non-identity of *diphtheria* and *croup*.

DIPHTHERIA is the disease *sui generis* which has now been briefly described. CROUP is not a disease—it is a name which authors sometimes apply to laryngo-tracheal diphtheria, and sometimes to several other diseases, which are essentially different in their pathology from diphtheria and from one another. If the word "croup" is to be retained in medical literature, it must, to avoid confusion, be applied to only a symptom—that is, to *stridulous breathing*, a symptom common to laryngismus stridulus, stridulous laryngitis, laryngo-tracheal diphtheria, and some other affections.

Most French physicians of the present day, adopting the views as well as the nomenclature of Bretonneau and Trousseau, apply the names *croup* and *vraie croup* to the membranous sore-throat which is the local manifestation of the general disease first called by Bretonneau, and now usually designated, *diphtheria*. They apply to laryngo-tracheal diphtheria, the terms *croup* and *vraie croup* (true croup), to distinguish it from the *faux croup* (false croup), the non-membranous affection in which spasm with stridulous breathing is the predominating feature. Many of them in this way include together the non-inflammatory spasmodic affection (laryngismus stridulus) with laryngitis, which is nearly always a stridulous affection in infants. It is the "inflammatory croup" of various English authors. Francis Home, of Edinburgh, represented it as a stage of membranous croup (diphtheria). In his famous little monograph, he only describes eight cases, and of these, five are tracheal diphtheria and three laryngitis. As no false membrane was seen in these cases, he—like many of his successors—jumped to the conclusion that the false membrane was absorbed, or its formation prevented by the treatment. For a long period Home's successors, British and foreign, continued to make the same mistake—and, indeed, till Bretonneau published the result of his clinical study, the error now adverted to was universally accepted as the truth. Cheyne mixed up tracheal diphtheria with laryngitis; and attributed the recoveries from the latter to the energy of the bleeding and purging—and the deaths from the former to the omission or imperfect adoption of that treatment. In 1810, the Emperor Napoleon offered a prize for the best treatise on "croup." All the competitors confused together laryngismus stridulus, common inflammatory laryngitis, and laryngo-tracheal diphtheria. Most English authors, when they write about "croup," really mean laryngitis and laryngo-tracheitis. If the term "croup" be so interpreted, it is of course totally different from "diphtheria," the disease which is the subject of this paper.

The following cases, though greatly differing from one another, are all typical cases of diphtheria, and they illustrate in a very interesting and instructive manner the most important parts of the clinical history of the disease.

CASE I. *Bronchial Diphtheria; False Membrane limited to lower part of Trachea, Bronchi, and Bronchial Tubes; Autopsy.*

In May 1871, at 11 o'clock at night, amid the roar of artillery, the turmoil and the peril of the closing period of the second siege of Paris, a despairing woman arrived from Neuilly at my ambulance with a male child aged about two years. She stated that it had been ailing for about six days, but that, from the dangers of the bombardment, she had been unable to leave her home to seek advice, till forty-eight hours before she came to me. She had then gone to a practitioner whose prescriptions she had since followed, and with whose sanction she consulted me. She attributed the child's illness and a severe catarrh, from which she herself was suffering, to residence in a dark damp cellar having been necessary for their safety for several days. Three leeches had been put on the upper part of the chest, and hot poultices of linseed-meal had been kept constantly applied to the entire chest, except over the leech-bites. A mixture containing ipecacuan and kermes had been ordered at the same time, and had been given regularly at short intervals up to the hour at which I saw the child. The symptoms which then presented themselves were semi-asphyxia, coldness of the surface, and a general absence of breath-sound on auscultation, and dulness on percussion, over the entire thoracic parietes. I recommended all medicines to be discontinued, a blister to be applied to the chest, and a teaspoonful of a mixture of brandy and beef-tea (one part to ten) to be given every hour till the child rallied; after which time—should it ever arrive, which I said was improbable—I ordered teaspoonfuls of milk to be alternated with teaspoonfuls of the mixture of brandy and beef-tea. The child lived till the evening of the next day, having for a short time rallied considerably under the change of treatment.

Some hours after death, I had an opportunity of making a hurried autopsy. The pharynx, larynx, and upper part of the trachea were free from false membrane. The lower part of the trachea, the bronchi, and larger bronchial tubes, were lined with a tough membrane, on carefully dragging which an arborescent continuity of the same was withdrawn from the smaller tubes. Peyer's glands and the solitary glands were prominent, and the mucous membrane around them was very vascular. Three lumbrici were found in the small intestine.

This is a case of diphtheria in which the formation of diphtheritic false membrane commenced in the lungs, and extended to the lower part of the trachea, but never existed on the mucous surface of the pharynx, larynx, and upper part of the trachea. The practitioner who treated the case before I saw it, considered—not unnaturally under the circumstances—that he was treating pneumonia; and I did not discover the nature of the disease till I made an anatomical examination after death. I only saw the patient once during life. The nature of the disease, as revealed at the autopsy, was also shown by a sister of the child, aged four or five years, dying of diphtheritic croup a fortnight after the fatal issue of the case now described. I saw her twice, and had no doubt as to the nature of the malady.

The cases in which the diphtheritic exudation does not first manifest itself on the pharynx or tonsils are only one or two per cent. The case now detailed is a typical example of the exceptional group to which it belongs. As the starting-point of the diphtheritic exudation was in the lungs, the case may be called one of "*bronchial diphtheria.*" "Bronchitic," or "pneumonic," are both prefixes which have been used in speaking of cases of diphtheria similar to that now described; but they are objectionable, as they suggest the presence of a condition commonly called "inflammation," which is not the pathological condition present in diphtheria.

By most German writers, the case would be called "croupous" pneumonia or "croupous" bronchitis. German physicians have abandoned the original signification of the Scottish word "croup," and now apply the term "croupous" to all affections characterized anatomically by the formation of superficial false membrane on subjacent mucous surfaces in a state of integrity. They speak of "croupous" nephritis, "croupous" gastritis, "croupous" bronchitis, and "croupous" pneumonia, In fact, in the medical literature of Germany, "croupous" and "pseudo-membranous" are synonymous terms; and some German authors, looking to the pseudo-membranous character of dysentery, call it "intestinal diphtheria."

The case now described is a case of diphtheria in which neither the pharynx nor larynx were affected, and in which the false membrane was limited to the lower part of the trachea, the bronchi, and bronchial tubes. It is a typical example of a class of cases which, though numerically small, constitutes an essential and very important part of the clinical history of diphtheria.

CASE II. *Pharyngo-laryngeal Diphtheria; Urine not Albuminous; Tracheotomy; A Short Respite; Death.*

On Friday morning, 31st December 1875, I received a note from General P., an English resident in Paris, asking me to visit his daughter, a child of about nine years of age. On my arrival at the house, situated in a healthy locality in the immediate vicinity of the Arc de Triomphe, I was informed by the family—that is, by the father and the elder sister of the patient—that the indisposition was "sore throat," associated with "biliousness." My first glance at the throat told a different tale. After examining the condition of the patient, I carefully investigated her health-history for the preceding week, writing down the material facts which I elicited, and which are here mentioned in corroboration of statements which I have made elsewhere in respect to the insidiousness of the invasion of diphtheria, and the kind of difficulty there is in determining the first day of an attack of this disease, even when the history of the stricken subject can be traced back into the routine of ordinary health.

On the evening of Friday, 24th December, the patient came home from a school in a neighbouring street, where she was a resident pupil, that she might pass the following day—Christmas day—with her father and sister. She made no complaint of being unwell; but, as she seemed to have "a cold in the head," her sister gave her a mild aperient in hot ginger-tea, and sent her to bed. Next day (Saturday), being "all right," in the opinion of herself and others, she attended a long Christmas service in the Rue d'Aguesseau English Church. On Sunday, 26th December, she again attended church. She made no complaint, was cheerful, and seemed quite well. On Monday, 27th December, in the morning, she returned to school apparently in perfect health. She there remained till the following evening, when she met, by appointment, her father and sister at a children's party, and returned home with them. Next morning, Wednesday, 29th December, she resumed her studies at school. As that day went on, it was observed that she was ailing. In the afternoon she complained of nausea and slight pain in swallowing. On the evening of that day, the cervical glands on both sides were observed to be swollen, and tender to the touch. An alum gargle was employed on the recommendation of the lady of the school. On the morning of Thursday, 30th December, there was increased pain in swallowing, more tenderness of the cervical glands, and a greater amount of nausea. There was also some vomiting of bilious matters. Under these circumstances, the patient was sent to her father's house early in the day, and not in the evening, as had been intended. In any case, the child was to have gone home on that day for the usual holidays of the season. She made little complaint; but she looked unwell, and admitted that she had sore throat, and no appetite. She took an aperient, and was put to bed. In the evening she had a warm foot-bath, and drank some ginger-tea. She slept badly during the night.

On Friday, 31st December, about 10 A.M. (as has been already stated) I made my first visit. The child was reclining in apparent comfort on a couch, reading, when I entered. Her countenance was good and her manner cheerful. The voice was not much affected, though appreciably husky and whispering after she had spoken to me for a few minutes. On both sides, the cervical and submaxillary glands were moderately turgid and tender. I made a very careful examination of the visible interior of the mouth and throat. The right tonsil was coated with a white pellicle, presenting an appearance which an inexperienced person might have mistaken for ulceration. There was likewise a small patch of similar pellicle on the arch of the palate, and on the back part of the pharynx. These three membranous patches were quite separate from each other. No exudation was visible on any other part of the mouth, throat, or on the body. No glands were turgid and tender except the cervical and submaxillary. Nothing abnormal could be detected in the lungs or heart by auscultation or percussion. The bowels were said to have been satisfactorily opened by the medicine. The urine was reported to be scanty. Some passed an hour before my visit, had a dark colour like the urine of jaundice; tested by heat and nitric acid it was found not to contain albumen. The tongue was slightly coated with a whitish slime. The pulse was 80, and remarkably feeble.

I announced that the disease was *diphtheria*—that the case was one of extreme danger—that there were two catastrophes to be dreaded within a day or two, namely, suffocation from obstruction of the air-passage, or sinking from sudden failure of the heart. The general symptoms, however, presented so little appreciable severity at this time that my statement was hardly credited. The father remarked that, as he had had experience in diphtheria, having lost by that disease one child, and nearly lost another, he had some grounds for hoping that I might be mistaken as to the nature and gravity of the case. I stated that the case was not without hope; but that the disease was diphtheria. I explained that, as the aspect of diphtheria varies in different cases, it was easy to understand his not recognising the present in the former malady.

The treatment I prescribed was a little good soup with some brandy every two hours, and at the alternate hours a cup of milk, an aliment to which the child was very partial. I explained the primary importance of stimulation and alimentation. My instructions on this head, I may here remark, were diligently carried out from my first visit till within an hour of the child's death. I prescribed a syrup of sulphate of quinine and iron, directing a dose to be given every four hours. Each dose contained two grains of the sulphate of quinine, and the sixth of a grain of sulphate of iron. I also directed a warm linseed poultice to be applied round the neck, and the mouth and throat to be washed from time to time with the spray of an aqueous solution of the glycerine of borax.

On my return in the evening I found the general appearance of the patient as good as in the morning, though the voice was a little more husky and whispering, and the greater part of the roof of the mouth was covered with a white pellicle. I ordered a continuance of the treatment prescribed in the morning, and requested to be sent for should a suffocative seizure, dyspnœa, or any other alarming symptom arise during the night.

On Saturday, 1st January 1876, about 9 A.M., I made my next visit. I was told that, about four in the morning, the breathing had been very much disturbed for a short time, but as this state had soon passed away in a tranquil sleep, I had not been sent for. The urine voided since my visit on the previous evening was scanty and turbid. It did not contain albumen. A portion which I had set apart for examination, with a view to determine its exact constitution, was thrown away by mistake. The appearance of the throat was very disquieting. The pellicle in one continuous mass covered the right tonsil, the back part of the pharynx, and the roof of the mouth. On the left tonsil there was no membranous coat, but only a gray speck as large as a split-pea; this speck had more the appearance of coloration than of exudation, though it was, no doubt, the latter in its very earliest stage. The breathing was not difficult. In reply to questions, the patient said, that for a few minutes at long intervals she had had a feeling "like suffocation," and that her greatest discomfort was a constant sensation of "pricking in the middle of her throat, and of there being something there which she would like to take out." I said —"Touch the place where you have this discomfort." She immediately placed her forefinger on the larynx.

The treatment was continued.

On Sunday, 2d January, between 9 and 10 A.M., I found a great aggravation of all the symptoms. The night had been restless, and at intervals the breathing had been disturbed by suffocative paroxysms. When I arrived, the breathing was declared to be then worse than it had ever been, and after my arrival the dyspnœa rapidly became so great as to be almost apnœa. After five or ten minutes of gentle muttering delirium, without any preceding attack of suffocation, struggling, or convulsions, she ceased to speak, rolled the head several times from side to side, and presented a turgidity and purpling of the face and neck. Within a few minutes, in fact when under my eye, she passed very calmly into a state of advanced asphyxia. The extreme gravity of this condition as now described was not of long duration. The severity of the asphyxial phenomena rapidly—I may almost say suddenly—diminished after a spontaneous fit of violent vomiting and retching, during which the ejecta were scanty, not consisting of more than three or four ounces of a thin slimy matter, in which floated tough membranous shreds, some of which probably came from the air-passages, but the greater portion of which had been detached from the tonsils, back part of the pharynx, and the roof of the mouth, as was evident by the patches of mucous membrane in these situations being denuded of their previous pellicular coating.

Before the patient had this unlooked-for respite, I had written a note to my friend Dr Jules Simon, requesting him to come with the least possible delay to give the benefit of his skill to the patient, and to divide with me the responsibility of a case of laryngo-tracheal diphtheria, in which tracheotomy could not long be delayed. It was during my father's absence to deliver my note that the respite occurred which has now been described. Dr Jules Simon was not at home, and did not join me in consultation till noon, till which time I never left the child. When waiting for Dr Jules Simon, I had the satisfaction of consulting in the case with my friend Dr M'Carthy, who agreed with me, that while the patient might at any moment require tracheotomy, the operation had for the time being ceased to be an immediate necessity. It was agreed that I should remain with the patient ready to act in accordance with any emergency that might arise, and to administer an emetic if the child made a rally under the influence of beef-tea and brandy, which for half-an-hour I gave with imperfect success, from everything received into the mouth occasioning spasmodic dysphagia. I was at last enabled to give the child twenty grains of the powder of ipecacuanha. It induced some vomiting and retching, with the result of more membranous shreds being ejected. Twenty minutes or half-an-hour of easier breathing than the child had had for many hours ensued. This temporary amelioration had nearly ceased when Dr Jules Simon joined me at noon.

We agreed entirely as to the nature of the case, the treatment, and the probable issue. First of all, we decided on a continuance of the treatment, alimentary and medicinal—a continuance of the beef-tea and brandy, of the ferruginous syrup of quinine, and of the local application to the interior of the throat and mouth. We then intimated that we believed life to be waning rapidly, but it was nevertheless our duty—founded upon our own experience and the experience of others in similar cases—to prolong life by the administration of aliments and stimulants, and, if need be, by tracheotomy, in the hope that during the prolongation so obtained, a spontaneous favourable change might occur, affording a fresh opportunity for the combined curative efforts of nature and art. We stated that our examination of the patient left us in doubt as to whether the diphtheritic pellicle had descended into the trachea and bronchial tubes; but that if it had, the hope of permanent advantage from tracheotomy was very small. We nevertheless emphatically declared that the operation was demanded, as it had often saved cases apparently as discouraging. The recommendation was accepted by the father, and half-past five was fixed for the operation, unless before that time an emergency arose which rendered delay dangerous, or unless an unlooked-for change for the better in the symptoms made it possible to dispense with the operation.

When we met at half-past five we found that matters were steadily and rapidly becoming worse. There had been only two or three suffocative attacks since noon, and they were of very short duration; but the asphyxia was nearly as urgent as at my morning visit. We said that there was no time to lose. The child was speechless, livid, and asphyxiated, when Dr Jules Simon operated at six o'clock.

The operation, easily and rapidly performed, was immediately followed by a cessation of the asphyxia. The patient a minute afterwards smiled, and with eager satisfaction drank a cup of brandied beef-tea which was offered to her. There was very little loss of blood. Not the smallest scrap of false membrane presented itself at the operation wound, nor in the canula, at the time of the operation or afterwards. The opening made in the trachea was, we therefore concluded, below the lowest part of the diphtheritically-coated mucous membrane. I left the patient at eight o'clock. She was then warm, comfortable, breathing nicely, and smiling answers which she could not speak. A slate having been procured, I asked her to write a reply to the question—"Do you feel better since the operation?" She wrote—"Much." And then, in reply to my second question—"Is there anything you wish to ask me before I leave you for a few hours?" She wrote—"May I have some milk?"

The patient's sister in my presence several times removed the inner canula before I left, replacing it, and locking it calmly and quickly. She was told at once to remove and wash the inner canula should any difficulty of breathing arise, and in any case to take it out once every hour till my return. This she did. At first the inner canula was found to contain black loosely congealed blood. There might be altogether about two or three teaspoonfuls of this black clotted blood removed from the canula, and from the sides of the operation wound. In colour and consistence it resembled black-currant jelly.

When I returned at midnight the father greeted me with the assurance that his child had made "solid improvement"—had regularly every hour taken her appointed dose of brandied beef-tea —had breathed well—and had slept calmly during the greater part of the intervals. I found the state of matters not quite so favourable as he believed. The respirations were more shallow and more rapid. The pulse, which was 88 when I left her at eight o'clock, was now 100 and feebler. Her countenance was pale, cheerful, and smiling. Using the slate she simply answered and asked questions. I said, "Do you feel much better now than before the operation?" She wrote (as at eight o'clock), "Much." I said—"Do you feel as well or better than when I left you at eight o'clock?" She wrote—"Just the same." One of the five or six questions she asked me was—"When shall I be able to speak?" When I said, not for a week, she smiled in a way which seemed to imply, "I am glad the time of speechlessness is to be so brief." I left the father and sister more satisfied than I was with the patient's state, and expressed myself to that effect, adding, however, that matters were in a far more hopeful state than when I made my morning visit.

I received a great surprise, as well as a very painful shock, on reaching the house at half-past nine next morning, to find that the patient had died suddenly about ten minutes before my arrival. She had taken nourishment an hour before death. Several times the breathing had been observed to be short and jerking, for a few minutes at a time, during the last three hours of life. This condition was always relieved by the brandied beef-tea. From the time of my last visit till her death, her countenance was pale; and just before death it had a ghastly pallor. She died quietly, in a state of syncope.

Within an hour after death Dr Jules Simon and I together withdrew the canula, and examined the trachea at the opening. Neither in the canula nor trachea could any false membrane be seen. There was nothing to prevent the free admission of air.

(*To be continued.*)

ARTICLE VI.—*On Conservative Aural Surgery.* By JAMES PATTERSON CASSELLS, M.D., M.R.C.S. Lond., etc., Fellow of the Faculty of Physicians and Surgeons; Surgeon to, and Lecturer on Aural Surgery at, the Glasgow Dispensary for the Diseases of the Ear.

"AURAL surgery is simply surgical common sense." So wrote one who dignified the practice of aural surgery by his matchless talent.[1] This aphorism, valued most by those who are competent to appreciate its truth, is yet, I venture to say, incomplete in its definition. As it stands, I fear that to many of my professional brethren, whose surgical talent is limitless, it may prove misleading, because hitherto, if one may be permitted to judge by results, surgical common sense, even combined with undoubted talent, has not coped successfully with the more important diseases of the ear. It is at this point, that the truth as well as the incompleteness of the aphorism as a definition of aural surgery becomes apparent. No amount of surgical common sense can possibly avail the surgeon in the domain of aural therapeutics, if he lacks the necessary special knowledge to guide him in the right application of the principles of his art in the treatment of the diseases of the ear. But right application means special knowledge; so that the practice of aural surgery consists, in fact, in the application of the general principles of surgery, guided by a special training to a special purpose. Therefore, were I asked to define aural surgery, I should be disposed to add to the pithy expression which I have already quoted, and to say that aural surgery is simply surgical common sense *rightly applied.*

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[1] Hinton, in his "Questions of Aural Surgery." Alas! this simple-hearted, self-sacrificing man, and consistent-living philosopher, has gone from our midst while the above was being written. He died on 16th December 1875, in the Azores. As his pupil and a close friend, I may be pardoned for revering his memory as I have done.

In claiming for aural surgery, and indeed for aural therapeutics in general, that it is in principle conservative, I am prepared for some degree of opposition from those of the profession who have not studied the natural history of ear diseases; but as those objectors cannot deny that *all* intelligent general surgery is possessed of this principle, and as aural surgery is clearly nothing but general surgery plus special knowledge, it is to me difficult to understand the grounds upon which they could rest their opposition. Doubtless, in general surgery the conservative principle, that seems always to underlie its application, does not at once arrest the passing gaze. If this is so in general surgery, it is otherwise in the treatment of the organs of special sense; then, this principle in the treatment of their diseases stands out quite prominently, so much so, indeed, as to ensure the notice of the most superficial observer.

In saying that aural surgery is eminently conservative, I do not mean that it is pre-eminently so over the special surgery of other organs of the body; but this I do say, that in their treatment, the principle of conservatism does not receive a more beautiful illustration, than it does in the early surgical treatment of some of the affections of the tympanum and Eustachian tubes. Nor is this statement without explanation. That explanation is, that ear diseases do not of themselves tend towards a natural cure, but to become progressively worse and more serious in their consequences; and further, that of the diseases of the ear amenable to our art, those alone are preventable and curable which admit of surgical treatment. Startling as these two propositions may appear, they rest, nevertheless, upon the reliable observation of trustworthy data; their truth, moreover, will be readily conceded by those, who have had special opportunities of studying the origin and process of evolution, that characterize the pathological changes in the ear. To those who have not had this opportunity, it will be sufficient for the purpose I have in view, to ask them to consider the probabilities and possibilities of simple inflammatory action in an organ whose position and construction, as well as vital relations, are similar to those of the ear.

Surrounded by the most important structures, and separated from them by osseous walls of varying thickness and degrees of development, it need not be a subject for wonder that serious and even fatal consequences can follow simple inflammation of the tympanic cavity; indeed, the closeness of the union that exists between the soft tissues lining this cavity, and the bones forming its walls, renders it an utter impossibility that the former can become diseased, however slightly, without the latter becoming involved in the morbid process. Thus it is that every congestion of the lining membrane of the tympanum is a periostitis, and every ulceration of it a caries of the osseous walls, that may lead to the most serious consequences. Now, inasmuch as these consequences are preventable by timely surgical interference, I claim for aural surgery that it is in truth conservative in aim, and that it does not exist, as a scientific pursuit, if it lacks this principle.

In order to satisfy those who may justly object to some of the statements which I have advanced, I propose to sketch the natural history of a form of ear disease which is typical of the congestive diseases of the ear generally, and one, moreover, with which every member of the profession is more or less familiar. I refer to that form of ear disease arising out of scarlatina, properly named exanthemal katarrh of the tympanum and its appendages. I have chosen this form of ear disease to illustrate the preceding remarks, not only because it may be accepted as typical of the congestive diseases of the organ, as I have already said, but because the general experience of it will bear me out when I speak, by-and-by, about its frequency and destructiveness; and also for the further reasons, that the surgical treatment of this ear affection is that which is alone successful, and that aural surgery first appears as a conservative art in connexion with its treatment. Let it not be supposed, however, for a moment that the principle of conservation in aural surgery is only applicable or visible in the treatment of this affection of the organ. In all the congestions of the tissues of the apparatus of hearing it is called for, and in none, for example, more urgently, than in those of the mastoid cells or external auditory meatus, out of which most of the serious and fatal consequences develop themselves—consequences nearly always avertable, by the timeous incision of the congested tissue in the initial stage of the primary disease.

Scarlatinal ear disease, the most destructive of all the ear diseases, and the one most frequently met with in general practice, arises out of the naso-pharyngeal affection, which is so marked a complication of this exanthem. The propagation of this congestion along the Eustachian canal into the cavity of the tympanum, and thence to the mastoid cells, must have been frequently witnessed, even by practitioners not specially interested in the practice of this department of our art. When this, the initial step in the causation of the ear disease, has taken place, its progress and development proceed with extraordinary rapidity. The Eustachian canal, as a result of this tumefaction of its tissues, becomes concentrically closed; in consequence, there is a rapid increase in the congestion of the tympanic lining membrane, owing to the disturbance which a closed Eustachian tube causes in the balance of the tympanic air-pressure. Inspection of the membrana tympani at this stage shows it to be, in general, unaffected by the congestion of the tympanic lining membrane. The temperature of the patient at this period is considerably increased towards evening, without a corresponding fall in it in the morning; there is much restlessness, rolling of the head, and more or less delirium, generally out of proportion to the violence of the general febrile attack. If now the interior of the ear is examined, the membrana tympani being still unaffected, except in a very slight degree, by the general congestion, it is generally possible to recognise through it the deeply purple-coloured tympanic lining membrane. As yet, there is no effusion into the cavity of the middle ear, although its outflow is imminent. Up to this point in the progress of the malady, it is possible by the timeous use of the knife to bring about resolution of the diseased action; failing this favourable and more desirable termination, the certainty nevertheless remains, that by this means the disease is deprived of its power to commit damage. This stage in the treatment of the disease, I call that of resolution and prevention.

The next step in the onward progress of the affection is more characteristic, is surrounded with more risk, and is of shorter duration than the preceding one; and because the resolution of the disease is no longer attainable, nor all the dangers to which it gives rise preventable, as they were in the earlier stage, I have named it the stage of preservation or cure. It is now that, owing to a marked increase in the hyperæmia of the tissues, and a diminution of the support usually afforded to the engorged vessels, there takes place an exosmosis of serous-looking fluid, which speedily fills the tympanum and mastoid cells. The pressure from this accumulation, constantly increasing as the fluid becomes greater in quantity, causes at last ulcerative absorption of one or several points in the parietes of the tympanic cavity or mastoid cells; a process, I may remark, that goes on with astonishing rapidity, and, as may readily be supposed, leads to serious consequences. Indeed, the future of the case is determined in great measure by this process, and the nature of the tissue in which it is set up; if it is the membrana tympani alone that suffers from the destructive process, less danger, both present and prospective, is likely to follow, than where the bony wall is broken down or perforated. The general symptoms from which the patient suffers in this stage of the disease. are much graver than in the former one; there is usually agonizing pain complained of in the intervals of freedom from this symptom; there is often wild delirium, and not seldom a state of coma, due to the pressure of the effusion upon the labyrinth. Pain, as a symptom, however, is not by any means a constant one; when it is present, it usually indicates periosteal or meningeal hyperæmia. If the membrana tympani is now inspected, it will be found no longer possible to see the purple-coloured lining membrane of the tympanum, by reason of the changes which have taken place in the membrana tympani itself. It is now of a bottle-green colour, with more or less bulging outwards; or it may assume a yellowish colour, if the contents of the tympanum have degenerated into pus.

It is in this stage of the disease that aural surgery, as a preservative, displays its advantages over the *laissez-faire* method of treatment. A free incision through the bulging membrane gives exit to the fluid, and arrests the destructive processes that may have been set up in some vital part of the organ. On the other hand, when the nature of the case is unrecognised, nature relieves herself by discharging the pus,—happily for the patient if it be through the membrana tympani,—with no other damage to the organ, even though a life-long otorrhœa is the consequence.

But it is not always that the case terminates so favourably; more often the violence done to the tissues by the long-continued pressure, or the outbreak of the imprisoned fluid sweeping away the essential parts of the organ, entails a life of misery in total deafness, or it may be death if the carotid or jugular are opened in the process of ulceration.

I have finished my sketch of the natural history of this form of ear disease. It is imperfect, but it is broad and true to nature, and, inasmuch as it differs from a description of other tympanic diseases in a few unimportant points, it may suffice to portray them as well.

If it is asked, Are the evils that arise out of this and other congestive affections of the tympanum avertable? it may be replied that they are; that all the structural changes and functional disturbance that follow such affections of that part of the ear, are preventable by the means that I have already pointed out. By them, one may, at the outset of the disease, procure its resolution; or failing this, in the latter stages, can, by free incision, undoubtedly limit its ravages.

To obtain the full benefit of the method of treatment here recommended, it is a necessary condition that the incision in the membrane be performed early in the progress of the disease; by so doing, you anticipate, it may be, by a few hours, the full violence of the disease; but the integrity of the organ is preserved by such prompt action, and you save the patient.

Pertinently enough, one may demand to know the symptoms by which this condition of the ear can be readily recognised. It is not "readily recognised" in the sense in which that expression is generally used. But it is really recognised without difficulty, by careful examination by means of reflected light and the usual speculum. This method of examination is absolutely necessary, and happily not difficult; otherwise, one must fail to diagnose this ear affection. Remember, there is no pain in the early stage of the disease, or so little as not to annoy or distress the patient or his attendant, whose attention, by the way, is likely enough to be engrossed by the more general symptoms of the fever. If one trusts to this complaint being made before examining the condition of the ears, the treatment here recommended may be too late to prevent, perhaps even to preserve, the tissues from serious damage. As a rule, when pain has set in, it may be concluded that the full violence of the disease has manifested itself, and that the results of the treatment which I propose will prove somewhat less satisfactory. Even in such a case, if the membrana tympani is intact, and the tympanum filled with fluid, do not hesitate to incise the former, with the hope that, though this proceeding may be too late to preserve, it is yet in time to save the tissue from utter destruction. The value of early incision in the treatment of the congestive affections of the ear, cannot be too much insisted on. Were it generally adopted, as I hope yet to see it, I do not hesitate to say, that in the future, by its aid, myriads of lives would be saved from the direst misery, even from death; for the lives that have been sacrificed in the past, and the consequences of this disease that I have pointed out, are as truly preventable by early incision, as certain fevers are by quina.

The operation which has been chosen, for reasons previously mentioned, to represent the principle of conservatism in this special department of the science of surgery, is that of parakentesis of the membrana tympani; the history of which is as follows:—[1]

Prior to 1649, aural surgery did not exist save in name only; all therefore was chaos in aural therapeutics. In that year, however, the first seed of scientific aural surgery was sown by Jean Riolan (died in Paris in 1657), and to-day we reap the harvest, which has been growing slowly in the two hundred years that have elapsed since then.

Riolan proposed to break down the membrana tympani in those incurably deaf by ordinary means, in order to cure them of their infirmity.[2] This proposal of Riolan's caused a storm of opposition, that lasted well nigh into the following century, and even yet, in some places, has not quite died out. But the seed was germinating; for in this century we find Willis and Cheselden experimenting on animals as to the effect produced by puncturing the membrane on the function and the life of the creature.[1] The first ray of conservatism seems to me to appear just at this time. Dienert, in France (the same who disputes with Busson), recommended the operation of incision of the membrana tympani to give exit to blood or pus contained in the tympanum.[2] The ray of light which Dienert emitted seems to have got extinguished; so I would decide, from the proposal made by Eli in 1760, to perforate this membrane, in order to permit of the passage of the sonorous waves—a proposal, then as now, followed by non-success. Eight years after, viz., in 1768, Portal (Précis de Chirurg. Pratique, t. ii. p. 480, 1768), and in 1775 Sabatier (Traité Complet d'Anatomie, vol. ii.), neither having seen the light referred to as coming from Dienert, are asking whether it would be allowable to make a small opening in the membrane, and if success would attend the operation in relaxed membrane.

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[1] The history of the operation in the text is abridged from my paper, "On Parakentesis of the Membrana Tympani," in the Glasgow Med. Jour. 1874.

[2] For the history of the origin of Riolan's suggestion, see *Encheiridion Anat. et Path. Lugd. Bat.* 1649, p. 290, and Sabatier, vol. ii. p. 127. In the lengthened notice of Riolan's works, in Portal's *Histoire d'Anatomie*, there is no mention of the subject, and in it the account of the Encheiridion is very exhaustive. I failed to procure a copy of the original work to consult; I have quoted it on the authority of Professor Schwartze, because I have no doubt of his accuracy; see also Arch. für Ohrenheil., b. ii. 1865.

Henceforth, the divergence from what I consider the true object of this operation becomes wider and wider. Carl Himly in 1797, and our own Sir Astley Cooper in 1801, recommended its performance for the cure of deafness due to closure of the Eustachian canal, just as Eli had done forty years before them. On the continent of Europe, at this time, the operation became quite fashionable for all forms of deafness, whether real or imaginary. Carl Himly (Commen. de Perfor. Mem. Tymp.), however, by a timely warning of the dangers attending the operation, put an end to the folly (Ueber die Durchbohrung des Trommelfells, 1810). From this time in Germany, till a comparatively recent period, the operation was frequently performed for such purposes as medication of the tympanum, inspection of its interior, passage of the sound waves, etc.

In France, Itard approached the subject from its conservative aspect, by proposing to remove accumulations of mucus from the tympanum by this operation. Saissy (1822) and Deleau (père) advise the operation, but do not describe the nature of the cases in which it is recommended. In 1843, the operation in France was abandoned, chiefly owing to the memoir of Hubert Valleroux (*Sur l'Abus et sur les Dangers de la Perforation du Tympan. etc.*, 1843).

In Germany, about this time, Kramer, Rau, Lincke, Frank, and others were operating with the same objects as their predecessors and contemporaries, and with like ill success; all, however, failed to see in the operation a valuable conservative remedy.

Retracing for a moment our steps in this history, we find that Sir A. Cooper had abandoned the operation on account of his ill success (although his labours were rewarded by the Royal Society's

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[1] Willis's experiments are criticised by Valsalva in his celebrated "Tractatus de Aure Humana," c. p. 5, 1740. For Cheselden's, see "Anatomy of the Human Body," p. 306, 1750.

[2] Itard gives this honour to Julien Busson aforesaid, but I fail to find any remark upon the subject in Busson's works. See Itard's *Traité des Maladies de l'Oreille, etc.*

Gold Medal). In the midst of the storm that these failures caused to rise against the operation, a voice cried once more on its behalf, as a conservative proceeding, and, alas! cried in vain. Saunders, whose voice it was, and whom I may style the father of English scientific aural surgery, proposed to perform the operation in cases of scarlatinal ear disease in order to limit its ravages. Although he does not describe, as I have done, the stages in which it may be employed with the greatest advantage, to him, nevertheless, belongs the honour of being the first to recognise in it the principle of conservation. His recommendation, and his remarkable work on the ear, at once a monument to his name and an honour to our country, fell unheeded; thenceforth aural surgery entered upon a period, lasting well nigh to our own time, which may be justly called the dark age of otology.

In this interval, there is no evidence that the operation was ever performed in England. Curtis, an ignorant pretender, certainly praises it in his book, but the grounds of his laudation are not discoverable; for it is not doubtful that he never had performed the operation himself. With the appearance of Toynbee passed away for ever the age of quackery in this department of our art, and aural surgery, as a scientific pursuit, assumed its true position. It is, however, remarkable, that this truly scientific aural surgeon did not recognise in the operation the principle which I have so often named. But so it is; for although he points out, with great clearness, a wider range of pathological states than his predecessors had done, in which it was likely to be serviceable, he seems never to have adopted it for the purpose recommended by the gifted Saunders, to whom I have already drawn your attention.

In 1865, the operation was revived by Professor Schwartze, of Halle, in the treatment of subacute mucous katarrh of the tympanum, in order to give exit to the mucus lying in the cavity of the tympanum. Shortly after this, it found favour, and was adopted for a similar purpose, by Von Tröltsch in Germany, Professor Politzer in Vienna, and lastly by our own distinguished otologist, Hinton.

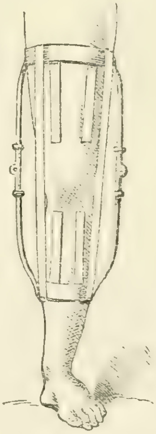
Till now, however, no one has insisted upon its value as a conservative measure—and, indeed, as the representative operation of conservative aural surgery; nor has any previous writer pointed out, as is done in this paper, the stages in which, if used, it manifests its preservative and conservative effects. In conclusion, it may be said that noble and important as the cure of disease may be, its prevention is a more worthy object of our efforts; in this respect, lies the chief strength of aural surgery. The true aim of aural surgery, whether represented by the simple incision of congested tissue or the more pretentious operation whose history I have just briefly noticed, is to prevent and to conserve.

ARTICLE VII.—*Surgical Note on a New Method of Applying Sayre's Knee-Joint Splint.* By Dr A. G. MILLER.

IN reading before the Medico-Chirurgical Society the histories of two cases of diseased knee-joint, which had been successfully treated by the application of the splint invented by Mr Sayre of Philadelphia, I referred to a mode of applying the splint, which I ventured to suggest as an improvement on the plan recommended by Mr Sayre. Some of my professional brethren who were present when I read my paper requested me to describe my method more fully. I venture therefore to lay this note before the profession, in the hope that it may be useful to some, who like myself have found it a great labour to put on and keep on Mr Sayre's knee-joint splint by the method that he recommends.

Assuming that any one intending to apply the splint is already acquainted with Mr Sayre's directions, I shall not describe his method, but merely state my own as briefly and plainly as possible.

I take four or more strips of strong adhesive plaster, each about one inch broad, and twice as long as the splint to be applied. These strips should be from 2½ to 3 feet long for a boy; and from 3 to 3½ feet for an adult (better too long than too short). Six or seven inches of the extremity of each strip should be warmed at the fire or gas, and applied from the head of the tibia downwards as far as the splint is to extend. (See Fig. 1.) These are secured in position by a bandage. The splint is then put on, and the strips are turned over its lower band (see plate) upwards towards the thigh, then round the upper band (see plate), and the free extremities, having been warmed by an iron, and if need be sufficiently shortened, are secured to the thigh as far down as the knee-joint. The whole is now firmly bandaged, and should be left to consolidate for four-and-twenty hours before the splint is extended (see Fig. 2).


[1] Fig. 1.

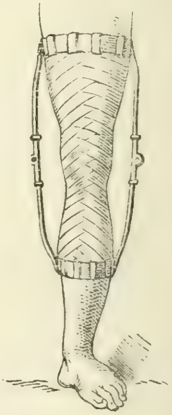


Fig. 2.

The method I have described gives a more uniform solidity to the whole apparatus, as will be evident from a comparison of Figs.

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[1] In Fig. 1, the upper ends should be represented as coming farther down, and the lower ends a little farther up.

1 and 2 with Figs. 3 and 4, which are Mr Sayre's. It is quite as easily put on, and is much less likely to slip. In fact, as has invariably been my own experience, when the plasters have once got a hold, the whole apparatus will remain firm till the strips are torn through at the place where they pass over the bands of the splint.

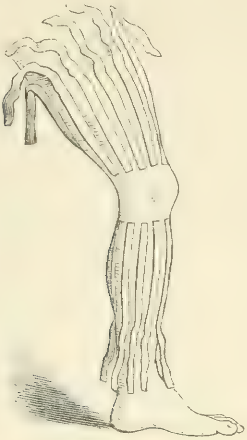

Fig. 3.

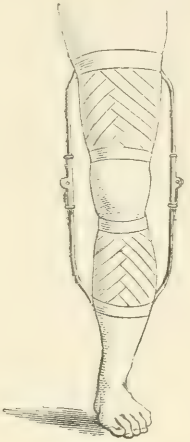

Fig. 4.

Article VIII.—*Case of Excision of the Elbow-Joint.* By J. P. Bramwell, M.D., L.R.C.S.E., one of the Visiting Surgeons to the Perth Infirmary.

The following case is recorded with the special object of showing how cut nerves, if quickly juxtaposed and maintained *in situ*, will continue their functions even before reunion has been accomplished—a fact perhaps but little known, and of no small practical import.

A. L., married, aged 30, has had one child, and is of a very strumous habit of body, as shown by an acute curvature of the spine in the upper dorsal region, and a carious elbow-joint, which last is killing her fast, by the formation of repeated abscesses, and by severe night pains. Her lungs are healthy.

With her consent and that of her friends, I excised the affected elbow-joint by a long posterior incision, aided by a short transverse one. The bones were disturbed as little as possible, but their articular ends were freely removed. From the matting together of the parts and some maladdress on my part, the ulnar nerve was completely divided. Its cut ends, however, were picked up, and brought into careful apposition, and so retained by transfixing each extremity with a needle threaded with carbolized catgut. The wound was then closed, and the arm placed on a broad, straight splint, so as to avoid any strain on the cut ulnar nerve.

Immediately after the operation the patient moved all her fingers freely, and, when pinched, declared that the sensation in each and all of them was alike.

There was little trouble with the arm, but the projecting dorsal vertebræ were so tender that she had to sit in a well-cushioned chair day and night for some weeks together. Being thus somewhat exposed, she caught a chill, which brought on an attack of acute Bright, with anasarca and albuminuria. This attack passed off in time, and in two months from the date of the operation she was able to return home, a distance of four miles.

On examination three months after the operation, I found free movement admitted by the false joint, and no emaciation of the muscles supplied by the cut ulnar nerve; indeed, the muscles never lost their normal bulk. Sensation all over the limb normal. The patient says, she can milk her cow, bake, and keep her house in order.

Remarks.—Division of the ulnar nerve is, I believe, by no means an exceedingly rare occurrence in this operation, and is followed by such a wasting of the muscles supplied by the divided nerve as to render the arm of but little use. Consequently primary union in the way described should always be attempted, bearing in mind that the carbolized catgut is the only binding material which offers any hope of success here.

Of the danger of tetanus we are not afraid, believing that nerves will bear with impunity much more interference than is generally supposed. In wounds with pointed cutting instruments about the wrist joint, where important nerves have been divided, an attempt should certainly be made to unite them by the catgut ligature, thus preventing subsequent atrophy, which a tedious faradization at some future period alone can rectify.

ARTICLE IX.—*A Case of Torpor of the Liver and Bowels from Birth, and consequent Persistent Constipation, in a Boy six years old. Recovery.* By CLARKSON CUTHBERT, M.D., F.R.C.S.E.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 1st *December* 1875.)

T. S., aged 6 years, a native of Brechin, has been from birth subject to a great torpor of the bowels and persistent costiveness. No passage of fæcal matter is ever obtained without the aid of strong purgatives or stimulating enemata, and often an interval of a fortnight elapses between each evacuation. Immediately after birth, and also occurring now and then for several days, he vomited a quantity of black matter mixed with mucus. Up to the age of three months, every napkin taken from him was stained with blood, which his mother said came from his bowels. During the period of infancy his stools were never of the usual gamboge colour. The stools all along have been of the colour and consistence of blue clay. The boy never had jaundice.

This state of matters appears to have had no appreciable effect on the patient's general health. He has always eat well; indeed, rather above the average for a young boy. Sleeps well; is active, and romps like any other child of like age. But latterly he has not enjoyed good health. When coming to Edinburgh from Brechin, he was obliged either to stand or lean during the whole journey of seven hours. He could not sit down without great discomfort.

The medical attendant who had charge of him was in the custom of administering purgatives, varying salts and senna, pills of aloes and soap, magnesia, castor-oil, etc.; and enemata, consisting of table-salt, castor-oil, and soapy water, were occasionally thrown up the rectum. No special medicines were prescribed with the view of acting on the liver. The chief intention of the medical man by the simple action of the remedies was merely to bring away matters lodging in the bowels.

Electricity was tried for a short time, but without success. No internal examination of the bowel had ever been made.

22d August 1875.—His general condition at this date is as follows:—He is a well-developed, strong-built boy; rather pale skinned, especially in the face; quiet, quick in his movements, and intelligent. The abdomen is enormously distended, evidently with solid matter, for it is quite hard to the touch, and percussion is dull over the whole surface. The position and size of the liver, and other abdominal organs, cannot be accurately defined.

The chest has the appearance of being deformed (pigeon-breasted). The shoulders are high; the head and neck are sunk down, whilst the sternum is thrown forward. Percussion elicits dulness extending to a considerable extent from the abdomen into the thoracic region. The heart and lungs seem to be pushed up out of their normal position. The impulse of the apex of the heart is distinctly felt above the nipple between the third and fourth ribs. The breathing is normal. The circumference of the chest across the ensiform cartilage is 26 inches; the circumference of the abdomen is 29 inches; and his weight is 56 pounds. He passes a normal quantity of pale straw-coloured urine, free from albumen; sp. gr. 103·0.

23d.—As a tentative measure, I ordered half a teaspoonful of compound liquorice powder to be taken in a little water night and morning, and ten drops of tincture of perchloride of iron to be taken in water three times a day. In the way of diet, to take oatmeal porridge and milk, oatmeal cakes, and ordinary common food. His food hitherto has consisted chiefly of bread, tea, soup, and potatoes. Oatmeal was not allowed, as it was considered to be too irritating.

27th.—The compound liquorice powder has had no effect in moving the bowels.

Introduced an elastic tube, half an inch in diameter, and pushed it up the bowel easily for about 10 or 12 inches; found no obstruction.

An enema, consisting of warm soapy water, olive-oil, and turpentine, was administered, but it only brought away a small quantity of feculent matter.

28th.—No movement of the bowels. Introduced my finger with much difficulty into the rectum, and I found a large accumulation of rounded lumps of very hard fæces. The rectum, as far as I could reach, was dilated into a pouch. I tried to bring away the mass by using the shank of a spoon, but failed; but making use of my finger, I succeeded in hooking the whole away bit by bit. It caused the poor boy a good deal of suffering. I now introduced the elastic tube, and pushed it up for about eight inches, and injected six ounces of warm olive oil. On my visit in the evening I found that the patient had passed nearly a third of an ordinary chamberpotful of soft, dark-blue stools. He felt much relieved, and very comfortable; the abdomen was softer, and evidently less in bulk.

As the iron and compound liquorice powder were not doing the patient any good, I stopped them, and substituted ten drops of Easton's syrup, to be taken three times a day.

29th.—During the night the patient passed nearly half a potful of light-blue coloured stools.

30th.—Passed another light-blue coloured stool last night.

The abdomen is much softer and flatter. The ribs can be more distinctly felt, and the circumference of the chest is now 24½ inches, being 1½ inch less than the last measurement.

Ordered the following powders:—

℞ Hyd. cum Cretâ, . . gr.xij.
Pu. Sacchar. alb., . . ℈i.

M., et divide in pulv. xij. Sig., One night and morning.

1*st September.*—On the 30th last month the patient passed three stools, and on the 31st two, all of a blue colour.

The general appearance of the boy is improving. His face is healthier, and he feels lighter in spirits and more comfortable. The chest is assuming a more natural appearance. On percussing the abdomen a little air is discernible. Administered an enema of warm olive-oil.

3d.—Three large stools were passed on the 1st; two on the 2d; one on the latter day was of a light ochre colour; and one was passed to-day also of a light ochre colour.

4th.—Administered an enema of warm olive-oil, which, during the day, brought away several copious stools of a light ochre colour.

6th.—To-day the bowels acted spontaneously. The stools were a light yellow.

7th.—Passed two naturally coloured stools.

8th.—Passed a natural stool this morning.

Administered an enema of warm olive-oil, which brought away several natural stools.

The boy's weight is now 50 pounds, being a loss of 6 pounds in 18 days.

To take only one powder in the day.

14th.—On the 10th, there were two natural motions ; on the 11th, two ; two very large ones on the 12th ; and to-day two large stools were passed.

16th.—Passed two large naturally coloured stools yesterday, and one to-day.

30th.—The boy appears to be quite well. He has generally two natural stools every day of moderate quantity. Easton's syrup is the only medicine he is taking now, which I recommended to be continued for some time.

The measurement of the chest is 24 inches ; on the 22d August, it was 26 inches. Whilst the abdomen, which on the same date measured 29 inches, has fallen to 23 inches.

Remarks.—This interesting case has two or three points worthy of notice.

1*st*, Persistent costiveness in a child, especially as seen in my patient, when not caused by mechanical obstruction, is very rare. I think there can be no doubt that this constipation, in great measure, was due to the absence of bile, the natural stimulus of the bowels. For, judging from the blue colour of the stools, and also looking to the early history of the child, the liver seems to have lain from birth, as it were, in a dormant or quiescent state.

In Reynolds's System of Medicine, vol. iii. p. 69, a case is there related of persistent costiveness from mechanical obstruction in a girl eight years old, who had long suffered from tendency to constipation ; that she had occasionally gone as long as three weeks, and at the time of her admission into hospital, there had been no relief of the bowels for seven weeks, and three weeks after sank from exhaustion. At the post-mortem examination the form of the distended intestines was distinctly impressed on the tense and thin abdominal walls, and on opening the abdomen the enormously enlarged colon was at first alone visible. The distention began at the cæcum, and extended to within two inches of the anus, where it ceased abruptly. In the greater part of its extent, the bowel measured from nine to ten inches in circumference, the greatest amount of distention being manifested in the sigmoid flexure. The bowel contained no flatus, but was completely full of thick semi-solid olive-green coloured fæces. These were more solid in the rectum than elsewhere, and immediately above the anus formed an indurated conical lump. "There can be no doubt," says the writer, "that the death of the child was due to the neglect of simple constipation; that the indurated fæcal lump above the anal orifice had formed a plug, which the bowel had been unable to expel, and which the accumulation of more and more fæces above and around it had served only to fix more securely."

2dly, It is somewhat remarkable that the child, under this anomalous state of matters, should have enjoyed such excellent health; and although experiencing great discomfort, and unable at times either to sit or lie down, could yet play and enjoy himself like other children. His appetite seems to have been rather voracious, but, so far as his own inclinations led him, he had no particular choice of food.

3dly, The mere purging treatment seems to have had no effect in relieving the poor boy. For although the bowels got emptied now and then by means of purgatives and enemas, yet, as nothing was done to secure the co-operation of the liver, it is not to be wondered at that so little progress was made in removing the constipation.

4thly, As regards the ultimate treatment of the case, I have very little to say; judging from the results, it seems to have been the proper one. I think this case brings out very clearly the valuable physiological action of mercury on the liver, and that small and often repeated doses is the best form for its exhibition. That it had a beneficial effect is evident, for no biliary secretion was ever seen in the stools of my patient, until the organ had been put under its influence. And I think, further, that Easton's syrup, besides being a general tonic, had a specific action through means of the strychnine in stimulating the muscular action of the bowels, and thus helping to keep up their spontaneity.

Olive-oil, I may remark, forms a most excellent enema; and it also has the advantage of softening and breaking down scybalous matters in the intestines, and thus favours their removal.

ARTICLE X.—*Remarks on the Inevitable and other Lacerations of the Orifice of the Vagina, and near it, in Primiparæ.* By J. MATTHEWS DUNCAN, M.D.

(*Read before the Obstetrical Society of Edinburgh, 26th January.*)

IN the outset I desire carefully to point out that anatomically the orifice of the vagina and the obstetrical perineum have no direct connexion. They are quite distinct parts, removed from one another by the structures forming the fossa navicularis. It is the orifice of the vagina which I declare to be invariably injured in the natural labour of a primipara. The perineum does not always escape laceration, but it frequently does so. It is not only anatomically that the orifice of the vagina is distinct from the perineum; it ought to be distinguished practically. When the head is on the perineum, or has passed beyond the outlet of the ligamentous pelvis—"out of the bones," as I have heard midwives call it—it is generally described as retained by a rigid perineum. But this description is plainly often inaccurate, for the perineum has yielded as much as can be expected or wished of it, and yet the head is not born. This condition is seen almost exclusively in primiparæ, and it is the orifice of the vagina, not the perineum, that is undilatable or rigid. [In such cases, it is habitual to deliver by the finger in the rectum pressing on the glabella in a proper direction, to force the head through the orifice of the vagina. This proceeding I object to, as I have elsewhere described, preferring the safer and equally efficient pressure through the skin in front of the coccyx or in the region of one or other sacrosciatic ligament.] To nearly complete the proof of my statement I have only to add, that in such cases the birth is often completed without injury of the perineum, the frænulum or fourchette even remaining entire. When the undilatability of the orifice and lower part of the vagina is great, and demands interference by the practitioner before the head has descended so far as to be amenable to pressure on the glabella, the forceps is applied; and, under such circumstances, I have often delivered without any injury of the perineum resulting, though certainly not without any injury of the orifice of the vagina.

Errors in the common descriptive anatomy of the vulva are so common, even in books of the highest repute, that, were there no other reason, it would be necessary here to give an account of it so far as my present subject demands, and, in doing so, the errors referred to will be made plain enough.

In the primipara, and it is her exclusively that we here describe, the orifice of the vagina is easily made out by observing the hymen (Fig. 1, *m*), as it presents, more or less injured by sexual connexion. The outer or larger margin of the hymen or its margin of insertion is the exact limit of the vagina, the margin of the vaginal orifice (Fig. 1, *e*, *e*, *e*, *e*, *e*, *e*). Close to the anterior or upper margin of the orifice, is the urethra (Fig. 1, *a*). At the sides of the orifice are the lower ends of the nymphæ, but there is quite a considerable interval between them and the vaginal orifice. Remote from the nymphæ, and closely adjoining the posterior margin of the vaginal orifice, are the openings of the glands of Duverney or Cowper (Fig. 1, *d*, *d*). These openings are in the fossa navicularis. This is a boat-shaped cavity (Fig. 1, *b*), lying between the orifice of the vagina and the fourchette or anterior marginal portion of the perineum. This cavity has really no shape; or rather it has that which is given it when it is examined by separating the labia majora, as in Fig. 1. Then, it is boat-shaped or navicular, and is big enough to admit the point of the little finger. The labia majora are not united posteriorly. They form separate piers (Fig. 1, *f*, *f*), parallel to and touching one another, and ending somewhat abruptly, their points looking posteriorly. They are connected by the perineum (Fig. 1, *h*, *h*), which forms their posterior commissure, and whose anterior margin (Fig. 1, *c*) is called frænulum, or more frequently fourchette. The perineum is described as extending from the fourchette to the anus (Fig. 1, *g*), and it has its central raphe.

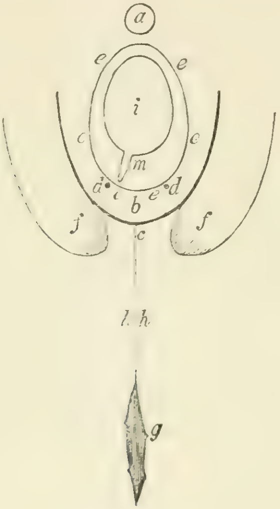

Fig. 1.

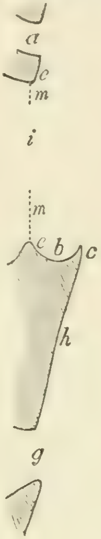

Fig. 2.

Fig. 2 is a plan of a vertical section in a sagittal plane, in which the lettering is as in Fig. 1. It is intended to make the description of these parts more clear.

After a natural labour the pudenda lose their characteristic appearances. The hymen is partially or completely destroyed. If the orifices of the ducts of Duverney's glands are not discoverable, it is impossible to say exactly where any part of the vaginal orifice is, except when distinct remains of hymen are seen. The distinction of the fourchette or posterior margin of the vulvar outlet from the margin of the vaginal orifice cannot now be made with any exactness. The labia majora have shrivelled, and the position of their projecting posterior piers is ill defined. If in a multipara the head is obstructed by the undilatability of the orifice of exit from the woman's body, then it cannot be said that the vaginal orifice is rigid and obstructing, for that part is not recognisable. It is the vulvar outlet that, by its undilatability, impedes progress. But the vulvar outlet from the genital passage is not the same thing as the vaginal orifice.

The lacerations of the vaginal orifice, as distinguished from those of the perineum, are very important. They may be the seat of pyæmic infection. Those in the anterior part of the vaginal orifice often produce hæmorrhage, which is occasionally, but rarely, even fatal, especially if the tear extends to the clitoris. Those on the posterior part of the vaginal orifice may be the commencement of greater lacerations, and they may be not only the commencement, but also predisposing causes of further laceration.

The lacerations of the vaginal orifice are not only to be distinguished from those of the perineum, but also from the other lacerations of the vagina. These are splits of the vaginal tissues of greater or less depth. They often are quite distinct from the upper vaginal lacerations connected with laceration of the cervix uteri, just as they are distinct from the lower vaginal lacerations connected with the vaginal orifice. They are produced by over-distension of the canal, and, as might be expected, are longitudinal in direction.

It would appear that, in the Darwinian progress of the species, the head of the fœtus has increased in size more rapidly than the orifices and passages through which it has to come have increased in size or dilatability. For it can scarcely be supposed to be a final arrangement that the cervix uteri should be torn so often as it is in giving passage to the child; and the same may be said of the lacerations of the vagina, the vaginal orifice, and of the perineum.

When, in a primipara, the head has distended the perineum, it is arrested by the vaginal orifice. During a pain, the head is propelled against the orifice in the line of its axis, or nearly so. A part of it acts on the orifice as a wedge projecting through it, and helping to dilate or distend it, or to burst it. If the orifice is unyielding, the pains push it as a whole more and more forwards, and among other consequences of this there is great elongation of the perineum. All parts of the circle are pushed forwards, the fourchette most of all. But even anteriorly there is great tension, and some degree of forward movement.

In this condition of matters, it is not one part of the orifice that is stretched, but the whole of it; and considering the shape of the fœtal head, it is at every point probably not equally, but nearly equally, stretched. The force tending to tear it, is acting nearly uniformly at all points of the circumference of the orifice. The condition of parts may be imitated by pushing a four-inch globe through a circular orifice of a considerably less diameter, in a sheet of indiarubber. The sides of the orifice are stretched in a longitudinal direction, while they are distended.

In this crisis, the distended orifice of the vagina in the primipara does not yield, at the same time retaining its entirety. It yields, and at the same time is invariably lacerated; and I know of nothing that can modify the tearing, unless such delay as may ensure all the dilatation possible before laceration commences; and such modification of the direction of the propelling pressure as may perhaps diminish the laceration of the weak posterior parts. When laceration has begun, further enlargement of the orifice is comparatively easily attained by further laceration. Whatever other laceration takes place, there is always some at or near the mesial line posteriorly. As I have already said, I have often seen primiparæ delivered, and even by forceps, without any perineal laceration, the fourchette being entire; but I have never in such a case separated the labia and examined the vaginal orifice without finding a laceration of its posterior part.

It is quite common to hear assertions of the complete absence of laceration in primiparæ, but I have never been satisfied that, in the cases referred to, a sufficiently careful examination has been made. To do it one requires a good light, an assistant, and a sponge.

It is scarcely necessary to remark, that the laceration of the vaginal orifice is frequently the first step to laceration of the fourchette or of the perineum more extensively. But all lacerations of the perineum are not mere extensions of tears beginning anteriorly. Lacerations commencing in the vaginal orifice or in the fourchette lead to many, probably to most, perineal lacerations, which may therefore be called secondary. But there are primary and essential or independent perineal lacerations, as is proved by the occurrence of central rupture.

The inevitable posterior laceration of the vaginal orifice is not the only one that occurs. It is often alone, but sometimes there are others. There is sometimes laceration of the side of the orifice anteriorly, where the parietal tuberosity has pressed, and this tear may extend into the adjacent nympha, and bleed freely. I think the left side is more frequently the seat of this tear than the right; and this is explained by a study of the mechanism of the delivery in a first or left occipito-anterior position. In it the right parietal tuberosity is born before the left; and when the left is passing, a greater ploughing pressure is exerted on the vaginal orifice than was exerted by the right tuberosity when it passed. This is a consequence of the greater dimensions of the part passing when the left tuberosity is in the orifice than of the part passing when the right was in the same orifice.

Sometimes irregular lacerations take place, little detached flaps being left hanging. These are probably produced by the tension at the orifice in a longitudinal direction, aided, perhaps, by a slight degree of friction, producing tears in a circular direction, and the subsequent detachment or pushing off of tags.

But the most important tears of this part are at its anterior margin, this special importance arising from the hæmorrhage which they occasionally produce. The tear takes place before the child's head is born, and the event may be diagnosed, if a sufficient interval of time elapses, by the period of its occurrence and its anterior position at the occipital region of the fœtus, blood being found in that situation. The exact mechanism of this tear I have not satisfactorily made out. I believe it sometimes affects only the side of the urethra, and then it is truly a laceration of the vaginal orifice.

The important laceration of the vestibule, extending backwards to the side of the urethra, and forwards to the clitoris, is not common, and difficult to investigate, and on this point my observations require further confirmation. I meantime designate this a laceration of the vestibule or triangular part bounded by the nymphæ, clitoris, urethra, and anterior portion of the orifice of the vagina. If all this be correct, then this vestibular injury is not a laceration of the vaginal orifice, and is rather analogous to the laceration of the perineum posteriorly. When the vestibule is distended and developed anteriorly, as the anterior part of the perineum is posteriorly and subsequently, the place of the fourchette is taken by a fold of the vestibule. The edge of this vestibular fold tears, and the tear may extend forwards to the clitoris or backwards by the side of the urethra. I must state that I have verified these observations by actual investigation, except the paramount one of the actual occurrence of the tear, which I have not seen at the moment of its production.

As to the important point, the order of time in which the lacerations of the vaginal orifice occur, I can give no good statement. Only it is quite sure that they all take place before the perineum is torn as an extension of the laceration of the vaginal orifice. When the perineum is so torn, the extreme tension of the whole vaginal orifice is of course annulled, and the liability to further laceration of it removed.

The inevitable laceration of the orifice of the vagina in natural labour in primiparæ takes place at or near the mesial line, in the posterior border of the orifice. It extends longitudinally, passing posteriorly along the vaginal wall, and anteriorly into the fossa navicularis. The laceration goes through a triangular mass of tissues, whose apex is the orificial margin, the part first torn, and whose base or longest side is the line of the middle of the bottom of the wound made. It is interesting, during labour, to watch the progress of this tear. The first little laceration or nick may be felt to be gradually increased till the full extent of the laceration is produced. If the head retreats, the wound may be felt to be of some depth, its sides collapsing; but if it only partially retreats, some tension remaining, and the finger is passed to examine, then the wound has no depth, but presents an extended, lozenge-shaped surface, whose margins may be recognised, and whose four angles are placed, one just behind the fourchette, one higher up in the vagina in the mesial line, and the other two at the now remote but formerly conjoined parts of the margin of the vaginal orifice at which the split or laceration began. After delivery the wound resumes comparatively small dimensions; but by proper manipulation it may still be shown, by separation of the labia (in imitation of the distension by the fœtal head), to have the characters above described as present during the passage of the head.

Before concluding, I shall enter briefly on the causes of this kind of rupture, although their consideration does not form an essential feature of the subject, because the causes are nearly identical with those of ordinary perineal ruptures.

There is no doubt in my mind that, in certain cases, there is what may be called rottenness of tissue, which destroys the power of the tissues to resist laceration or bursting. In some women, and occasionally at least very markedly in the syphilitic, this condition is very easily demonstrated. It is a condition also of many inflamed tissues, and this is exemplified in the perineum.

The element of time is important in the study of causes, for there is general consent that a part rapidly dilated may give way, while the same part slowly dilated may be induced to yield and dilate without tearing or giving way. This is almost a truism, illustrated, as it may be, in all tissues, living or dead.

The element of time cannot be fully considered without simultaneously attending to the element of force or pressure. Of course, all tearing is effected by a pressure superior to the resistance. A pressure slightly superior to the resistance may only prove itself to be so after many attempts and considerable lapse of time, and, it may be, without laceration, except what is inevitable; whereas a greater pressure may rapidly overcome the resistance, and will probably do so by producing one or more lacerations.

The element of force or pressure cannot be fully studied without taking into consideration the child's head or the body propelled against the resistance. Now, it appears to me, that the head must be regarded as a blunted or pointless wedge. A point is not required, because there is already a passage which needs only farther dilatation, not new formation. The wedge-shaped head will be more efficient in proportion as it is sharper; and this condition of sharpness will be increased by three circumstances that are subjects of frequent observation. First, it is evident that the overlapping of bones will increase the sharpness and power of the wedge. Second, the smaller the fœtal head, the sharper its wedge-like shape. This may offer an explanation of the circumstance demonstrated, or nearly so, by Hecker,[1] that the mere size of the head is not in direct proportion to the frequency of laceration of the perineum. Thirdly, the recent caput succedaneum may act as a wedge on parts not otherwise subjected to any distending pressure, and it may also increase the sharpness of the already previously acting wedge-shaped portion. Of course, in this theory, it is taken for granted that the edge of the caput succedaneum is below, or acting on the part to be dilated. Lastly, the wedge-like shape will be changed by projections such as the parietal protuberances, which will diminish the sharpness of the wedge so far as these particular parts are concerned, and increase the likelihood of laceration by these parts.

The direction of the propelling power is also to be taken into account, and may be regarded as always producing greatest pressure on the posterior part of the passage, the part which has to undergo the most extensive dilatation. Referring to the commonly used term, circle of Carus, we may point out that this direction of special pressure is a matter of course; as it is true of all curvilinear motion, the counter pressure on the convex side being required to produce the deviation from a straight course.

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[1] Klinik der Geburtskunde, S. 143.

The part that is weakest will always, *cæteris paribus*, be the first torn. The actually weakest is probably the posterior mesial part; and, as has just been stated, it has to endure the disadvantage of being subjected to greatest pressure.

If a laceration is inevitable, treatment to prevent it can be of no avail. But all the lacerations of the orifice of the vagina are not inevitable; and that one which is so, may be treated with a view to prevent its extension beyond the inevitable degree.

Two important elements in the causation of laceration are susceptible of modification with therapeutical objects by the practitioner, namely, time and direction. The accoucheur can prevent the precipitate expulsion of the child, and its attendant evils. He can, by supporting the perineum, modify the direction of its advance, and resist any undue pressure posteriorly or inferiorly arising from the curvilinear motion of the fœtus.

Part Second.

REVIEWS.

A Treatise on Surgery, its Principles and Practice. By T. HOLMES, M.A. Cantab., Surgeon to St George's Hospital. With 400 Illustrations, chiefly by Dr WESTMACOTT. London: Smith, Elder, and Co.: 1875.

THE name of the author is in itself a sufficient guarantee, that in this goodly volume of 900 pages will be found much good work, careful and copious compilation, judicious arrangement and selection, with some original investigation of a high order. Long experience as editor, teacher, and hospital surgeon, gives Mr Holmes the right to speak, and assures him an attentive audience wherever English surgery is known and valued.

A more difficult point to settle might be as to whether such a work as this is wanted at all. Have we not text-books enough and to spare, from the great System which bears Mr Holmes's name, to Cooper's Dictionary, and the systematic works of Spence, Bryant, Pirrie, Gross, Erichsen, Druitt, etc.?

Taking the work up with an idea that it was not needed, we have laid it down with the feeling that it does supply a want. It may do to take the place so long and worthily occupied by Mr Syme's beautifully short and compact work on the "Principles of Surgery," now, alas! out of print, and, for examination purposes, out of date. This work of Mr Holmes is one which we believe may be fairly recommended as a text-book for students in their first year of surgery. It is more compact and readable than Erichsen or Gross: it contains principles, and hence is more suited for the "man of one book" than either Bryant or Druitt; and though it has not the invaluable originality and clinical richness of Spence, it is more suited to the comprehension of a junior student, especially of an English school; and it being in one volume (certainly a very heavy one) in itself is an encouragement to the unfortunate student of the present day.

We have neither space nor heart for the careful analysis of a systematic work. The doctrine of inflammation and the pathology of morbid growths are apt to pall upon the cloyed palate of even the most enthusiastic teacher, but by dipping here and there into the heart of the work, we may fetch up some samples of the kind of instruction given and views held.

On the best means of dressing wounds, Mr Holmes is still undecided, and no wonder. He says, "Surgeons are now further from agreement as to what is the best plan for dressing a common incised wound than they probably were in the days of Podalirius and Machaon. One sees in the atmospheric air the universal medium of contagion; another teaches that wounds do best when freely exposed to the air; a third repudiates all special methods of treating wounds, and thinks that they do better with water-dressing than with any other application."—Page 17.

Mr Holmes approves of the antiseptic system, as under its use immunity from surgical fever, etc., is more common than under any other system. Yet, in a footnote, he takes care to tell us that such an opinion does not necessarily involve any adhesion to the "germ theory of disease," and that, in fact, he believes the opponents of this theory have the best of it. This being the case, we venture to prophesy that Mr Holmes will not long either practise or believe in what he is pleased to call antiseptic surgery.

On the subject of arrest of hæmorrhage, Mr Holmes says that while acupressure and torsion are both perfectly reliable methods of arresting hæmorrhage, his private opinion is that neither are so convenient, safe, or likely to promote rapid union of the wound as the carbolized catgut ligature. "In London, acupressure is, so far as I know, universally disused after a fair trial, and torsion is only practised at a few of our hospitals."

We may note here, by the way, that this sentence is an example of a haste and inaccuracy in composition, which is unworthy of Mr Holmes's powers. "Universally disused" is at least an awkward way of stating the fact, that the use of acupressure has been discontinued; while the "only practised" is either a grammatical error for "practised at only a few," or an absurdity; "*only professed*" would have expressed his meaning better.

In the chapter on Injuries of the Head, we find some excellent remarks on their importance, insidious nature, and the occasional anomalous character of the symptoms, which render diagnosis and prognosis difficult. In this chapter, as well as elsewhere through the work, we have some excellent thermographs; the ones illustrating concussion and meningitis are very characteristic and instructive. At page 280, in the middle of the chapter on Injuries of the Lower Extremity, we come upon a sentence on the treatment of compound fractures without amputation, which we think gives a most erroneous view of present surgical opinion. "Without dogmatizing on the subject, I believe I express the general opinion of surgeons of experience in saying that careful attention to the instant closure of the wound, graduated support, and exact apposition, are more essential to success than any special application." This may be all very well in mild cases, in which the external wound at once closes, and which from the first are practically simple fractures; but many cases may be saved by judicious treatment, in which the instant closure of the wound would be very bad practice indeed, causing tension, pain, burrowing of matter, and thus necessitating extensive and perhaps numerous incisions. The free use of drainage-tubes and other means of relieving tension is a better practice.

The chapter on Tumours contains an attempt of a classification which will be easy to the student, and retain much of the old nomenclature, and yet be founded on the microscopic structure and relations of the elements of the tissues composing the tumour. The task is difficult, and Mr Holmes has not, we think, been altogether successful; but it is much easier in such cases to criticize than to suggest tangible improvements. The whole question of the classification of tumours requires revision, and is in a transition stage.

The chapter on Ulcers is deficient in details of description, and especially remarkable in its arrangement, which certainly will not assist the memory of the student, or stimulate the scientific imagination, which delights in an orderly arrangement of pathological phenomena.

Yet in the chapters on Bones, Joints, and Bloodvessels, the most carping critic will find little to object to.

Take it as a whole, the work of Mr Holmes gives very high promise. If he can only prevent it from getting too large in successive editions, it seems a work which, from its completeness, generally admirable arrangement, judicious illustrations, and copious index, will prove a most useful one. Containing, as it does, accounts of special branches—eye, ear, syphilis, operative surgery, and bandaging—it will be useful for the type of student who may be described as *homo unius libri*, at least during his first year of surgery. If Mr Holmes succeeds in getting over all the ground he travels in this work during a winter course of systematic lectures, all we can say is, he is a fortunate and wonderful man.

A Practical Treatise on Diseases of the Eye. By ROBERT BRUDENELL CARTER, F.R.C.S. London: Macmillan and Co.: 1875. Pp. 591.

FROM the excellent quality of the papers with which Mr Carter has from time to time favoured the profession, we were led to form high expectations of this work, and we may admit that, on the whole, our expectations have been fulfilled. What, in our opinion, this treatise chiefly lacks are arrangement and classification of its contents, and the use of headings to indicate at a glance the subject under consideration. It is chiefly on account of these defects that we cannot thoroughly recommend it as a text-book to the student, or a book of reference to the practitioner. We would rather consider it as comprising a series of essays on most of the important diseases of the eye. This book, however, is well worthy of study by the oculist, or by the surgeon who has a desire to acquaint himself with the recent progress of ophthalmic surgery. It is eminently readable. We have the author's views expressed most openly, and in language both terse and choice. In it we have the results of Mr Carter's experience and observation freely revealed, and the only treatment, as a rule, spoken of and recommended is that which he himself has found most beneficial.

The chapters we consider the most interesting, and perhaps the most valuable, are those on the Principles of Ophthalmic Therapeutics, on the Principles of Ophthalmic Surgery, and on the Uses and Selection of Spectacles; while, in the chapter on Diseases of the Conjunctiva, the important subject of follicular conjunctivitis or contagious ophthalmia is ably discussed.

While in the main we thoroughly agree with Mr Carter in his views, there are a few points which we noticed in perusing the volume in which we think he differs from most ophthalmic surgeons, and in some of which we are inclined to believe he is mistaken. We shall shortly refer to the more important of these, with the hope that our candid criticism may be of service in rendering a second edition more perfect.

The first few pages contain a short but clear description of the Anatomy and Physiology of the Eye. In it there is only one remark to which we would take exception. It is that, "in position, the iris, when seen in profile, should be vertical, forming the chord to the corneal arc, and should not be projected forwards towards the cornea." This, we believe, is only the case when the pupil is abnormally dilated, or perhaps in very aged persons in whom the lens has become very much flattened; as in the normal eye, and when the pupil is of medium size, a considerable part of the posterior surface of the iris lies upon the anterior capsule of the lens, and receives from it an inclination forwards. This is particularly noticeable during the early years of life, when the lens is more convex; while, on the other hand, after cataract-extraction, the plane of the iris is markedly altered, having become vertical, and the depth of the anterior chamber increased.

In his chapter on Ophthalmic Therapeutics, Mr Carter refers to a certain form of iritis which is apt to occur in abnormally sensitive and irritable persons, and gives several excellent illustrative cases. In one of these, the tendency to relapse or to recurrence under circumstances that tend to exhaust the nervous system, was particularly well shown. He is inclined to attribute such inflammatory attacks directly to derangement of the nerves which govern the nutrition of the parts concerned. While we agree with the author that cases now and then occur in which the ordinary course of disease is modified by peculiarity of constitution requiring special remedies, we scarcely think that a certain variety of these cases should be separated as a class and called the neurotic forms of ophthalmia. We have already ample division and subdivision in ophthalmic affections, and we should not make new divisions unless some decided benefit may be expected from so doing. All that is necessary is, when describing the diseases which are specially modified in nervous, irritable patients, to refer to the existence of such peculiar cases, and indicate the treatment applicable. Unless this method be adopted, we would require to revert to one of the old methods of classification, and group diseases according to the supposed constitutional peculiarity on which they depend—a system that could not be carried out in practice.

Mr Carter is a staunch disciple of Ricord, and has very strong views as to the absolute necessity of counteracting the syphilitic poison by a prolonged course of mercury—mercury given "with due intermissions and precautions for something like twelve months." Less than this will, he admits, leave the patient in most cases apparently well, but the disease is apt to manifest itself at some future time. While we are of opinion that mercurials are very serviceable in many syphilitic affections, we would hesitate to subject a patient to so long a course of the drug, fearing that the remedy might eventually prove worse than the disease. Preparations of extensive mercurio-syphilitic caries, which abound in museums, but which fortunately are now rarely to be met with in practice, teach us to be cautious in the use of a medicine so potent for evil as well as good. Iodide of potassium, Mr Carter believes, is more rapid than mercury in its influence upon the symptoms actually present in cases of secondary and tertiary syphilis, but probably possesses little or no power to cure the systemic malady, which sooner or later manifests itself in the same or in some modified form; while, on the other hand, mercury produces its first action more slowly but more effectually as regards the syphilitic taint. He therefore recommends the administration of iodide of potassium in case of rapidly advancing disease so as quickly to check its progress, but in less acute or chronic affections, the use of mercury.

Mr Carter favours the use of anæsthetics in almost all operations on the eye—including extraction of cataract. Having in some cases of extraction seen evil effects, endangering the success of the operation, result from the retching and vomiting during or following the administration of chloroform, we are rather chary of the use of it or its rival ether, and only have recourse to anæsthetics where we have fidgetty, nervous patients to deal with. Our author advocates the use of ether in preference to chloroform, as being as effectual an anæsthetic, and having the advantages of safety and less frequent occurrence of sickness. With regard to the question of the relative tendency to the production of sickness, statistics are much needed, as opinions on that point differ. Thus, the American ophthalmic surgeon Dr Williams, in the third edition of his manual on Diseases of the Eye, expresses the opinion that chloroform is less apt to produce sickness than ether. Mr Carter gives a representation of a most complicated-looking ether-inhaler, constructed by Mr Hawkesly. We have a horror of all such ingenious contrivances, and are convinced that the simpler the instrument is with which the anæsthetic is administered, the safer and more efficacious will it be found in practice.

Mr Carter, in very strong language, condemns the operation of removal of the eyelashes and their bulbs, along with a narrow portion of the margin of the lids, in cases of incurving of the tarsal cartilage. We quite agree with him, that this is not the best operation for that condition of the lids; but we think he very much exaggerates the evils produced by the operation, and we have certainly seen cases in which very good results were thus obtained. We are astonished that he has not tried Mr Streatfeild's operation (grooving the tarsal cartilage) in this affection, which in our hands has proved most successful, and which is so manifestly fitted to counteract the incurving of the cartilage.

In the chapter on Diseases of the Cornea, we were astonished to find the following expression of opinion:—"Superficial clouds, the result of changes in the external epithelium, may be completely removed in course of time; but the opacities left behind by loss of true corneal structure are essentially permanent, though they may undergo some apparent diminution by the disappearance of superficial turbidity surrounding them, and some actual diminution by the gradual contraction of the cicatrix." We can, from observation in many cases, aver, that very considerable loss of corneal substance may take place, and the new opaque tissue that replaces it become clear and transparent—so long as perforation of the internal elastic lamina has not occurred.

While speaking of inherited syphilitic keratitis, Mr Carter throws out the suggestion that the syphilitic virus may have been introduced into the system by vaccination with lymph yielded by a child who has itself inherited the disease. The considerations he brings forward in support of this doctrine are not easily controverted; and whether we consider this view proven or not, we at any rate approve of his recommendation, that a vaccinator should repudiate as a source of supply, not only those children who are themselves obviously syphilitic, but also those whose parents have suffered from the disease. The evils that may result from the tainting of the system with the poison are so distressing, that no precautions can be too great to avert the chance of its inoculation.

Mr Carter very correctly condemns the use of vinum opii as an application in the early stages of interstitial keratitis, under the mistaken idea that it acts as a sedative. It is undoubtedly a stimulant application. We, however, do not agree with him when he says that he is not aware of any useful purpose which can at any time be fulfilled by its application to the eye. We have found it very serviceable in stimulating the clearing of corneal opacities after inflammatory action has subsided, and often a grateful and useful application in chronic conjunctivitis.

We consider the application of vinum opii to the surface of the eye as likely to prove beneficial in hastening the disappearance of corneal turbidity as the sub-conjunctival injection of a tenth or a fifth of a grain of chloride of sodium in five or ten minims of water, of which treatment he speaks so favourably. In either case benefit, we believe, arises from stimulating the flow of blood in the vessels that nourish the cornea; and of the two methods the application of the vinum opii is certainly the simpler and pleasanter, and, we should imagine, not less effectual.

A new method of performing iridectomy for artificial pupil, and a modification of the ordinary operation of abscission for staphyloma of the cornea, both devised by the author, are described. We have not practised either operation, nor seen either performed. They are, however, ingeniously contrived, and will probably prove useful modifications of the ordinary operations in certain cases.

Mr Carter treats of the various operations whereby cataract may be extracted in a masterly manner, and avows a decided preference for the modified linear operation of Von Graefe. He, in a most able and amusing way, criticises the various modifications of Von Graefe's operation that, bearing their authors' names, were freely introduced to the notice of the public and the profession, but which did not in principle differ materially from that of Von Graefe, and also points out forcibly the defects of the procedure of Dr Kuchler of Darmstadt. As an example of the author's style of writing and powers of criticism, we may be allowed to extract the following passages in which he discusses these matters :—

"During this period, when real and important changes were being effected in the methods by which cataracts were removed, the surgeons engaged in the work had many followers who made changes which for the most part were only apparent. It is hardly possible for two pairs of human hands, especially if endowed with different degrees of skill, to execute all the steps of a complicated operation precisely in the same way; and so it came about that each of several operators found it more convenient to himself, more suited to the requirements of his own eyes and fingers, to deviate in some minute point of detail from the practice of somebody else, of whom, nevertheless, he was in the main an imitator. Such deviations were often described, in pamphlets or in medical journals, as the 'modifications,' or even as the 'operations,' of those who first had recourse to them; and in this way a certain amount of cheap fame appears in some instances to have been secured. Of such changes there were none really worthy of record, or which possessed more than a fleeting and personal interest. They mostly suggested themselves as natural correctives to some kind of manual incapacity; and will suggest themselves again, as it were instinctively, to those who share the defects of dexterity in which they had their origin.

"The avowed disciples of Von Graefe were not long in discovering that the operation of their master had left them between two dangers. He taught them to make an incision which was barely anterior to the iris, and altogether in the sclerotic coat; and in following his instructions they sometimes wounded the ciliary region and produced sympathetic ophthalmia of the other eye. In seeking to avoid this risk by an incision in an anterior plane, lying partly or entirely in the cornea, they became liable to cause sloughing of the cornea, and they not unfrequently produced a change in its curvature which required to be corrected by a cylindrical glass. There arose, therefore, much division of opinion with regard to the best place, within narrow limits, for the incision; some operators preferring to keep well back and away from the cornea, while others cared little for the cornea if they could avoid encroaching upon the ciliary region.

"Amid the turmoil of this controversy there appeared one innovator who had the merit of being original. Dr Kuchler, of Darmstadt, in 1868, published a pamphlet, in which he advocated a section made across the middle of the cornea in its horizontal diameter. After having extracted the lens through this opening, it was his practice to immobilize the eyelids by enveloping the whole head and face in a sort of helmet of plaster-of-Paris bandage, which was not removed for several days, and under which he believed that uninterrupted healing would be likely to take place. His publication is a curious example of how much may be said by an ingenious person in defence of a proceeding of which, at first sight, it seems difficult to speak seriously. His arguments are conclusively refuted by his statistics, which extend to twenty-eight cases of his peculiar operation. In this number he had one case of suppuration of the wound, six cases of adhesion of the iris to the cicatrix, seven cases in which the whole of the cortex could not be removed, six cases of loss of vitreous humour, and ten cases in which a second operation was required for the establishment of a pupil. The amount of sight gained by the patients is not stated with exactness; but it is stated that the transverse corneal cicatrices offered no impediment to vision.

"From certain internal evidence afforded by Dr Kuchler's pamphlet, I cannot help believing that his love for the transverse section arose from the readiness with which it could be accomplished by the surgeon, rather than from any advantages which it offered to the patient. I am not aware that he has had a single faithful follower; but a few operators have been tempted, possibly by the fatal facility of his procedure, to come very near to it; and among these both Mr Liebreich and Dr Warlomont have practised, and have even advocated, transverse section of the cornea as a first step towards the extraction of cataract. These gentlemen have not made their incisions exactly in the horizontal diameter, but they have gone a little above or below it. Mr Liebreich has described the incision downwards as constituting his peculiar method; but the incisions both above and below the pupil are mentioned and discussed by Kuchler, who advances cogent reasons for preferring the exact diametral line; and who anticipates, in favour of his own operation, all the arguments which have been advanced in favour of the others. It would be waste of time to examine these arguments, because transverse corneal incisions stand self-condemned on *a priori* grounds. They have the single recommendation that it is very easy to make them; and they might perhaps be attempted with advantage by a benevolent traveller who was sojourning among a savage tribe, or by an ophthalmic surgeon upon whom the infirmities of age were creeping, or by one who was prevented, by the natural quality of ambi-sinistrousness, from employing better methods with ordinary prospects of success. Even in such cases Lord Melbourne's pithy inquiry, 'Couldn't you have let it alone?' would be likely to suggest itself to reasonable men. As a matter of first principles, an incision through the front of the cornea must in a large proportion of cases be followed by adhesion of the iris to some part of the cicatrix; and adhesion of the iris, even if vision is for a time restored, entails a perpetual liability to the occurrence of destructive morbid changes."

While many able ophthalmoscopists describe as distinctive affections, "optic neuritis," "perineuritis," and "choked disc," we have always confessed our inability in practice to distinguish the one from the other, and we are glad to find ourselves supported in this view by Mr Carter. But while he favours the term choked disc as the one most applicable to these affections, under the impression that all the appearances presented in these cases may be referred to obstruction to the circulation in the retinal veins, we prefer the term optic neuritis, believing the condition to be one of true inflammation. In support of our view, we would point to the character of the exudation, which is of a more solid nature than the serous transudation we would expect from simple obstruction, and to the fact that the retina itself retains its transparency, and is often not impaired in function, and the vitreous humour free from turbidity. Were the condition one of obstruction, we would expect the over-distended vessels to part with some of their contents where the resistance was least, and that certainly is not in the firm structure of the nerve.

We have already occupied so much space that we cannot do more than mention one or two other points which we had marked for criticism. We are convinced of the existence of "tobacco amaurosis," in which Mr Carter expresses little faith. We also cannot understand why our author does not admit that any of the described forms of retinitis are inflammatory except the pigmentary—which form appears to us to present less of an inflammatory character than almost any of the others. He, for example, speaks of syphilitic retinitis as "the resentment of a retina encumbered by syphilitic deposit;" but he might in like manner, and with equal propriety, speak of syphilitic iritis or choroiditis as the resentment of an iris or a choroid encumbered by syphilitic deposit, and deny their inflammatory nature.

We notice that the adaptation of a glass scale, to be worn for a time as the precursor of a well-fitted artificial eye, is recommended a week or ten days subsequent to enucleation. We think little is to be gained by this procedure, and prefer adapting an artificial eye at once, after an interval of at least five or six weeks. In cases of extensive convergent strabismus, we do not see what can be gained by his recommendation not to divide the two internal recti muscles at one operation.

The last chapter, that on the Uses and Selection of Spectacles, is, as might be expected, excellent, and we would only take exception to one sentence in it—that in which he recommends that short-sighted children should be made to wear spectacles habitually. On this point we are aware much difference of opinion exists, and we freely admit that the stooping posture, which is almost inevitable in marked myopia when spectacles are not worn, is productive of evil; but, on the other hand, the exertion of the power of accommodation necessarily associated with the use of spectacles in such cases, is, we believe, equally injurious; and we have been impressed by the fact that many of the worst cases of myopia that have come under our observation have from an early period of life worn spectacles.

The illustrations (which are almost all original) and the printing are excellent; and while we have pointed out what we incline to believe defects or doubtful doctrines, we cannot but acknowledge the great general worth of the book, and recommend it as one creditable to English ophthalmic surgery.

Researches into the Antagonism of Medicines: Being the Report of the Edinburgh Committee of the British Medical Association. By JOHN HUGHES BENNETT, M.D., F.R.S.E., etc., etc., Chairman and Reporter. London: J. and A. Churchill: 1875.

FEW names were better known or more honoured in medicine than that of the late Professor John Hughes Bennett. For a quarter of a century he was a distinguished Professor in the University of Edinburgh. He was a successful teacher, and, as a clinical instructor, he was equalled by few, and surpassed by none. He was an able debater and an eloquent writer, and by his death scientific medicine has lost one of its truest friends.

The work now under review is the *last* which bears the honoured name of John Hughes Bennett, and is a most valuable contribution to the "advancement of therapeutics."

The labours of the Committee extended over four years, and during that time they investigated the antagonistic properties existing between—

1. Hydrate of chloral and strychnia.
2. Sulphate of atropia and Calabar bean.
3. Hydrate of chloral and Calabar bean.
4. Hydrochlorate and meconate of morphia and Calabar bean.
5. Sulphate of atropia and meconate of morphia.
6. Meconate of morphia and theine.

7. Meconate of morphia and caffeine.
8. Meconate of morphia and guaranine.
9. Meconate of morphia and infusion of tea.
10. Meconate of morphia and infusion of coffee.
11. Extract of Calabar bean and strychnia.
12. Hydrate of bromal and atropia.

We strongly recommend this volume to all who take an interest in the advancement of scientific medicine.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LV.—MEETING IV.

Wednesday, 2d February.—Dr GILLESPIE, *President, in the Chair.*

I. *Dr Foulis* showed a DERMOID TUMOUR OF THE OVARY. The tumour was removed, by Dr Thomas Keith, from a girl aged 13. The girl was very tall, and had an unusually large quantity of black hair on her head, about the genitals, and on the pubes. She had never menstruated. The tumour consisted of one large cyst, in which were a number of secondary smaller cysts. In one part of the tumour a piece of the parent cyst, about six inches square, presented on its inner surface an appearance very similar to the skin on a person's forehead, and it was covered with numerous minute soft hairs. In different parts of the tumour locks of black hair, three or four inches in length, grew from the epithelial surfaces of some of the smaller cysts, and several pieces of bone could be felt embedded in the substance of the tumour. Dr Foulis considers that such structures—viz., hair, bone, teeth, etc., in the ovary—have nothing whatever to do with abortive fœtal developments, as some have supposed, but that they are peculiar connective-tissue growths derived from the stroma of the ovary.

II. CASE OF SWALLOWING OF ARTIFICIAL TEETH. *Dr Craig* exhibited, for Dr G. Dickson, an interesting specimen, though not exactly pathological, viz., part of a set of false teeth swallowed and passed safely. The following are the details of the case, as given by Dr Dickson:—Mrs I. called about 6 o'clock A.M. on Christmas last, stating that she had, while asleep, swallowed her artificial teeth. On passing a taper-pointed bougie, an obstruction was encountered at about the level of the middle of the sternum. Emesis was tried, but unsuccessfully. An expanding hair probang was then used, with the result of pushing the obstructing material into the stomach. She was then told to go home, and cut up a quantity of sewing thread into finger's lengths and swallow it in thick porridge. She was also ordered to eat plenty of figs and raisins, and not to take any purgative medicine. This treatment was continued for eight days. Each morning she felt a gnawing pain at the pit of the stomach, which was always relieved by food. On the 1st January, the pain began to shift, and on the forenoon of the 2d, she felt a severe cutting pain at the anus while at stool, when she found that she had passed the teeth wrapped up in the threads. She had been asked, in the event of her passing the teeth, not to clean them in any way till examined, but before my arrival she unfortunately had washed them, leaving only a few of the threads adhering. The hollows were filled with a substance like figs. The set consists of a gold plate 1¾ inches wide by 1 inch deep, and measuring 2½ inches round the front, with hooks at each side for fastening to the natural teeth.

My reason for using the thread was, that having seen hawks "cast" up the bones of mice neatly wrapped in the fur, I thought that in a similar way the peristaltic action of the stomach and bowels might envelop in indigestible fibrous matter, such things as pins, bones, teeth, etc., inadvertently swallowed. I tried the plan with good results some time ago in a case of pin-swallowing.

Cotton, tow, or similar substances, might suit even better than thread.

III. *Dr M. Duncan* then read a paper ON THE ALLEGED OCCASIONAL EPIDEMIC PREVALENCE OF PUERPERAL PYÆMIA OR FEVER, AND OF ERYSIPELAS, which appears at page 774.

The President said that the Society was much indebted to Dr Duncan for his able communication on such an interesting subject. He was convinced that the non-epidemic nature of puerperal fever and erysipelas had been thoroughly shown. The statistics brought forward were so large, and extended over such a long period of time, as to be perfectly trustworthy. He believed that the error arose from people mistaking what an epidemic was. The statistics showed, that the fact of there being a large number of cases did not prove any epidemic nature. The last table given, viz., that on the influence of season on puerperal fever, erysipelas and scarlatina, demonstrated how much the first two differed from the last. All knew the popular dread of these three diseases when a midwifery case was under treatment. He doubted, however, from what Dr Duncan had brought forward, if this fear was well grounded.

Dr Ballantyne had paid some attention to the literature of epidemics in general, and out of four or five hundred epidemics, the details of which he had examined, he could only find two of puerperal fever. Neither of these had any connection whatever with scarlet fever or erysipelas.

Dr Taylor thought that Dr Duncan deserved a unanimous vote of thanks for his paper. The amount of labour required for such a production was enough to overawe all who knew anything about it. He thought, however, Dr Duncan had failed in some respects. For instance, he had not defined the term epidemic. He indeed told them what it is not, viz., not a mere increase of frequency, but had not defined what an epidemic really is. There were necessarily fallacies in all statistics, and in the present case one special fallacy was that the statistics were based on the death-rate. There were, however, often epidemics without many deaths, and erysipelas was peculiarly one of these. No one regarded it as a deadly disease, and probably not one case in seven proved fatal. Hence, statistics based upon the death-rate of such a disease must necessarily be fallacious. Dr Duncan's reasoning seemed also to be at fault when he assumed that because we never had a *great* epidemic of certain diseases, therefore they were not epidemic in their nature. He might as well argue that because a stickleback is not a salmon, therefore it is not a fish. His own statistics show that in 1860 (?) erysipelas raged far more virulently than in the few years which preceded or followed it, and without any explainable cause. That year, therefore, we had an epidemic of it, although a small epidemic. A few weeks ago Dr T. was, at the same time, attending cases of erysipelas in three widely separated districts in Edinburgh. The disease broke out simultaneously, without any possibility of intercommunication or contamination, and he believed that many of his professional brethren had also cases at the same time, also without contagion or infection to propagate them. That looked very like an epidemic on a small scale, yet the death-rate was not affected by it. As to the relation between scarlatina, puerperal fever, and erysipelas, he believed that Dr D. was right in his opinions as regards the non-identity of the poisons, and he had recently been attending a case which brought out some interesting facts relating to this. The patient was a woman in her third confinement, one of whose children had at that time scarlet fever. No bad effects followed in the mother's case, although the child slept with her. Then another of the children was attacked, and finally the baby. At the time of the fourth confinement of the same patient, the father was seized with erysipelas; but just as in the former case, no bad results followed as far as the mother was concerned; and had it been possible for either erysipelas or scarlatina to convey puerperal fever, she could hardly have escaped.

Dr Bruce said—I have listened with great pleasure to the paper read by Dr Duncan to-night. I would wish to make some remarks from a practical point of view, having been actively, and I may say extensively, engaged in midwifery practice during the last twenty years. I have often heard about epidemics of puerperal fever occurring at different periods, but have never observed anything of the kind in my own practice. I have had comparatively few cases of puerperal fever, and do not remember of ever having had more than one or two in close succession. The nearest approach to anything of the kind I ever had, happened in July 1874, when, during the course of one week, out of eight confinements, there was some amount of fever present in no less than seven. One patient died of well-marked puerperal fever; one turned out to be a case of pelvic cellulitis, which ended well; the others were more or less of an ephemeral nature, lasting only for a few days, without presenting any urgent symptoms, yet, under the circumstances, giving rise to some anxiety. It is worthy of mention, that scarlet fever was very prevalent at the time, but I am not aware that other practitioners had a run of similar cases. The mere fact, however, of these cases occurring almost simultaneously, by no means proves anything of an epidemic character, as I myself might have been the medium of communication. The result of my experience leads me entirely to coincide with Dr Duncan in his opinion that there is no such thing as an epidemic of puerperal fever.

Dr Angus Macdonald believed that the tardiness in criticism was not due to any want of interest in the paper; but rather to a sense of the enormous amount of work it involved, causing a feeling of difficulty as well as of diffidence on the part of the members in criticising such merit. He thought it was certainly one of the ablest papers ever brought under the notice of this Society since his connexion with it, and one that would do more to clear the minds of obstetricians than all the discussions in the Obstetrical Society of London last year, or the voluminous writings published within the last eighteen months. After Dr Duncan's demonstration, he did not see how any ingenuous mind could escape from acquiescing in the accuracy of the results so ably stated and fairly deduced. He had been specially struck with the latter quality in reference to the suggestion of the possible fallacy, that in the twenty-seven years from which Dr Duncan had gathered his data, there might have been no epidemic of puerperal fever in London. But in that case, we may fairly conclude that no epidemic of this disease is ever likely to appear. The whole question had very important bearings as to obstetricians. If any man entered on obstetric practice with the idea of puerperal fever being epidemic, then he would be very often tempted to neglect the due care of such cases when they happened to occur in his practice (and there was no obstetrician of large experience who did not occasionally meet them), and allow them to die unaided rather than run the risk of causing the death of his other patients. This idea of the infectiousness of puerperal fever was often extravagantly put, viz., that an obstetrician absorbed in some way or other the puerperal fever poison into his system, and might give the disease two or three months afterwards to a woman in labour through his secretions, so that there was no security as to the period when an obstetrician who had the misfortune to meet with such a case in his practice, might safely return to duty. If, however, he only believed in its contagiousness being similar to that of other putrid diseases, then, by means of care, cleanliness, carbolic acid, and Condy's fluid, he would be able to attend puerperal fever or scarlatinal cases without resigning his practice for two or three months. He did not believe that there was any more risk in obstetricians attending puerperal fever cases than in surgeons attending erysipelas ones, if they trusted, like the latter, to cleanliness and disinfectants.

Professor Simpson agreed with the previous speakers as to the high value of Dr Duncan's communication. It was the more valuable, too, because it had come at a time when the relation of the puerperal female to febrile disorders was demanding attention. If he might have asked for any definition, it would have been for a definition of puerperal fever. For it was a wide term, and comprised a considerable number of febrile conditions. He thought this was important; but it did not invalidate Dr Duncan's results. He believed that scarlet fever was more dangerous to a puerperal patient who had not previously had that disorder, than to one who had before suffered from it. This would explain the result in Dr Taylor's case. And, in addition, they could not draw conclusions from negative observations. As to the connexion between puerperal fever and erysipelas, it would be a mistake to conclude, from the non-identity of the maximum of each disease, that it was perfectly safe to attend a woman in her confinement after attending a patient with erysipelas. He had always wondered that those who alleged that puerperal fever was epidemic, had not brought forward statistics. He had recently been reading Lusk's work on Puerperal Fever in Bellevue Hospital, New York, and had come to the conclusion from it, that puerperal fever was not epidemical, but was due to nurses, etc., being the media. Such could be seen in any hospital. During the last three months the conditions in the Maternity Hospital of their own city were unhealthy. Thus, one patient, admitted in her eighth month, died after her confinement of scarlet fever. She was removed to the Infirmary; but although the child died, none of the other women were affected. They had, however, given each of them doses of sulpho-carbolate of soda. In three other cases, where there were respectively double pneumonia, pelvic cellulitis, and measles, there was no connexion whatever in the way of an epidemical relationship, and no bad effects followed as regards the other patients. If, however, these had suffered from no previous attacks of scarlatina or measles, the effects might have been serious.

Mr Chiene would have liked to start with some definition of an epidemic from Dr Duncan, although he was not prepared to give one himself. He wished, however, to put this query: Is it not necessary in such an inquiry that we should, for a true comparison, have the numbers of those liable to each disorder? He believed there was a fallacy in Dr Duncan taking all the population as being in all cases liable to the diseases, the epidemicities of which were compared in the tables. Thus, in regard to cholera, all the population were probably liable, and therefore the curves were right. But the curve of scarlatina was not, simply because the whole population were not liable to scarlatina. The same was the case in regard to puerperal fever.

Dr Arthur Mitchell asked in what light Dr Duncan regarded the seasonal exaltation of the mortality from puerperal fever, in October and November. The average of thirty years showed this exaltation to be emphatic. This involved the frequent, if not constant, occurrence of the same phenomenon among the individual years which made up the thirty. Otherwise there must have happened in some one or two of the years a very unusual exaltation. But it had been shown that nothing of this kind had taken place. He asked if these steadily recurring seasonal exaltations of the death-rate from puerperal fever were to be regarded in the light of periodic epidemics. In the case of deaths from the disease in question, he thought the seasonal increase strongly and sufficiently marked; but it would be easy to find a disease—diarrhœa, for instance—in which the seasonal intensification of the death-rate was vastly more pronounced, and, at the same time, absolutely steady from year to year. The difference, indeed, between the mortality from diarrhœa in the cold and in the hot months of almost every year was as great as the range between whole years of lowest and highest fatality from diseases which we commonly regard as violently epidemic. Are we then to treat the annual prevalence of diarrhœa in July and August as an annual epidemic? It must be borne in mind that a curve for diarrhœa constructed for a series of years on the same plan as the curves for the diseases in diagrams 1 and 2, would be a steady curve showing trifling fluctuations, and would entirely conceal the enormous disturbances which regularly occur within each of the years. This regularity of occurrence, Dr Mitchell thought, took all these annual seasonal exaltations of mortality out of the region of epidemics. If so, since it may eventually be found, when we have observations extending over a much longer series of years to deal with, that great special morbilities, manifested in certain whole years, are as steady and periodic in their appearance, and as readily accounted for, as great seasonal morbilities appearing more or less steadily within each year, we may be forced to the conclusion that there is no such thing as an epidemic—such, at least, as we understand it,—a thing, that is, which presents itself at irregular and uncertain intervals, and without known cause. Dr Mitchell concluded by expressing his great admiration of Dr Duncan's paper.

Dr Temple Wright thought Dr Duncan had acted with caution in not giving a definition of the term epidemic. In his own experience in India, alleged epidemics often turned out false. Dr Wright then went on to give the results of his observations as to the effects of cold, damp, etc., on disease in tropical climates, and concluded by expressing his high appreciation of Dr Duncan's paper.

Dr M. Duncan was quite unable to criticise Dr T. Wright's excellent remarks, as he had had no experience of tropical diseases. As to the various effects of cold, etc., in these and other parts, he had, however, seen some excellent papers. Of these, one had been read to this Society, showing that below a certain latitude, heat was the great cause of increased mortality, while with us cold assumed this evil prerogative. He was not quite sure if he understood what Dr Arthur Mitchell had said, but he thought he did. His (Dr Mitchell's) argument was best illustrated by diarrhœa. That disease had a tremendous curve-peak in July and August in London. The occurrence of that tremendous prevalence of diarrhœa could be surely predicted. It was annual. Further, its cause was known with at least a high degree of probability. Whether this yearly very great degree of prevalence should be called an "epidemic" of diarrhœa, he could not decide, for he knew of no trustworthy definition of that word. But whether it were so called or not, the case or thesis he (Dr Duncan) wished to prove remained unshaken,—as indeed Dr Mitchell admitted it was; for the July and August diarrhœa was a regular yearly occurrence, and probably accounted for, and therefore not an ordinary epidemic. Besides, it had a decidedly epidemic character, in so far as its then prevalence was out of all proportion to its prevalence at other times. In all these important respects it had nothing in common with puerperal fever. There was of this disease no extraordinary yearly prevalence at a particular time. No one had pretended to account for the imaginary epidemics of puerperal fever. The rises and falls in puerperal fever were never extraordinary, never beyond the range of a general yearly average, as was the case with diarrhœa. No disease was, indeed, better than diarrhœa, for illustrating the difference between puerperal fever and a disease of occasional extraordinary or epidemic prevalence. Every year, in the case of diarrhœa, there was proof of this particular kind of epidemic prevalence. The prevalence of diarrhœa was, no doubt, seasonal, and there was a seasonal prevalence of puerperal fever and of erysipelas, as Buchan and Arthur Mitchell had shown; but the seasonal prevalence of these last diseases required the accumulated experience of thirty years to bring it out, and then not perfectly. This kind of prevalence was totally unlike any epidemic, whether of cholera, or scarlet fever, or diarrhœa. It was slight, not extraordinary, never going beyond the range of an average of the whole year. It had long been known and ascribed to the injurious influence of cold, and never confused with the imaginary epidemics, which he (Dr Duncan) had shown never to occur, being mere imaginations. Moreover, if this seasonal variation were a sufficient source of epidemics in puerperal fever and erysipelas, then all diseases were epidemic, as all had a seasonal variation; and this was a *reductio ad absurdum.* There was no accepted definition of the word "epidemic," and he was not foolish enough to make one, and run the risk of marring his demonstration. He had taken the practical plan, viz., that of comparing erysipelas and puerperal fever with undoubted epidemics and non-epidemics. The criticisms of Mr Chiene were undoubtedly acute; but he had anticipated and answered them in his paper. The second diagram had indeed been drawn up expressly to avoid the fallacy attachable to large numbers. He had shown carefully that from 1871 to 1873 there was no epidemic of scarlet fever, and that when this disease is non-epidemic in the population, it affects nearly the same numbers as puerperal fever. As to the number liable to puerperal fever, suppose it was five or six thousand, the popular opinion was that a very large proportion died in an epidemic, instead of it being as he had really shown. But he wished further to call Mr Chiene's attention to the fact, that in the beginning of his paper he stated "that he recognised numerous errors and imperfections in his statistics." Mr Chiene had only pointed out one of these. While the whole population was perhaps liable to cholera, perhaps not more were liable to scarlet fever than to puerperal fever. This, however, was one of the uncertainties or imperfections he would be delighted to get over. The remarks of some of the other speakers had borne out what was said of the philosophers of this country, viz., that they could not theorize without drawing practical conclusions. He had been extremely amused with the philosophers of their Society jumping at once to practice, as his friends Dr Macdonald and Professor Simpson had done, trying how best they could push his conclusions to their patients' advantage. As to the rest of the speakers, he could only say, owing to the lateness of the hour, that he agreed with them where they agreed with him, but entirely differed where they did not.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXV.—MEETING IV.

Wednesday, 12th January.—Professor SIMPSON, *President, in the Chair.*

I. *Dr Matthews Duncan* exhibited a specimen of FIBROUS TUMOUR OF THE ROUND LIGAMENT. The tumour was of about the size and shape of a hen's egg. It lay quite free in front of the right broad ligament. The right round ligament could be traced to its surface, where it ended in the capsule. Its pedicle was small, thick, about a quarter of an inch broad. The structure of the tumour was that of a dense fibroid, with numerous cretaceous portions near its centre, and having a fibrous capsule from the round ligament. Dr Duncan remarked, that practically such a tumour was interesting, as it might be mistaken for the ovary, if felt during life.

II. *Dr Matthews Duncan*, in the absence of Dr Linton, showed a preparation of an ENTIRE OVUM, in a case of partial placenta prævia.

III. *Dr Bruce* mentioned having attended a case of labour in which the UMBILICAL CORD measured thirty inches only, and yet was coiled three times round the infant's neck.

IV. *Dr Bruce* exhibited a SIX MONTHS' ANENCEPHALOUS FŒTUS, presenting the usual characteristics. The specimen had been preserved in the chloral-hydrate solution for a fortnight, with success.

V. *Dr Matthews Duncan* then read a paper, entitled AGAINST THE PENDULUM MOVEMENT IN WORKING THE MIDWIFERY FORCEPS, which appeared in the February No. of this Journal, p. 683.

Dr Bruce concurred entirely with Dr Duncan's views regarding the inutility of the pendulum movement, which, in the earlier years of practice, he had adopted, but now discarded.

Dr Keiller had long taught and practised direct traction rather than the pendulum movement in working the forceps. He was under the impression that the practice of pendulum movement had generally been given up. In certain cases, however, owing to peculiar positions of the head, it was justifiable, indeed called for. In working the forceps, the natural efforts towards expulsion must be watched and assisted, and the best guide to the safe use of the forceps was the so-called "mechanism" of labour, without the study and knowledge of which it was difficult to apply and to use the forceps aright. The natural passages and efforts were by no means straight, and we must guide them accordingly. It ought always, however, to be used with great care. In the main he agreed with the views brought forward by Dr Duncan.

Dr Angus Macdonald regarded pendulum action as a mistake in theory, and unnecessary in practice. Formerly he had tried it, but now had entirely given it up.

Dr Young, Portobello, *Dr Wilson*, and *Dr Gordon* also made some remarks.

Professor Simpson had listened with much interest to the paper, and thought that Dr Duncan had done good service in bringing out so clearly the grand action of the forceps as a tractor. But if he had understood him aright, he (Professor Simpson) thought Dr Duncan had gone too far in averring that forceps never could and never should be used as a double lever. He knew that one blade of the instrument could be worked so as to move the head by a lever action; and, on a model, the head of a doll could be retracted by means of forceps worked as a double lever without traction. The cases certainly were rare where this kind of action of the forceps was required, but he (Dr Simpson) had found it helpful in starting the head in cases where it had got wedged in the pelvic cavity, and he was not prepared to have the pendulum movement discarded for every case and for ever. With regard to the injuries and indentations of the fœtal head, which had been attributed to this mode of working the forceps, it should be borne in mind that the child's head was sometimes damaged where no pendulum movement had been impressed on the forceps, but the instrument had been employed simply for traction with compression.

Dr James Young agreed generally with Dr Duncan. He said that it had been the practice of his late father to adopt the pendulum or lateral motion in many cases of difficulty, especially in primiparous women, and he believed such was the custom among obstetricians twenty years ago. During the earlier years of his own practice, he had been in the habit of having recourse to the pendulum movement. Now he trusted only to steady traction during each maternal pain, as he considered there was less likelihood of bruising or lacerating the soft tissues.

Dr Duncan then replied.

VI. OBSERVATIONS DEMONSTRATING THE INFLUENCE OF DIGESTION IN THE MOTHER UPON THE FREQUENCY OF THE FŒTAL PULSE, by Mr Charles Naylor. Communicated by Dr Matthews Duncan.

Dr Matthews Duncan commended the paper, and observed that it contained important evidence upon the question of the communicability of influences from mother to fœtus through substances introduced into the mother's stomach.

Dr Angus Macdonald inquired whether any observations had at the same time been made on the maternal pulse.

Dr Bruce thought the paper of interest, as the conclusions deduced from Mr Naylor's observations might tend to invalidate the results arrived at by those who consider the rapidity of the fœtal pulse an index of the sex of the child. Unless the question of the influence of maternal digestion on the fœtal pulse be taken into account, it would obviously vitiate these results.

Dr Keiller remarked that the observations of his old pupil and Maternity House-Surgeon, Mr Naylor, were interesting, but not free from question, on the ground that the mother's pulse—as Dr Macdonald had questioned—had not been specially noted at the times referred to in Mr Naylor's communication. It was, moreover, well known to the practised obstetrician, that even during the application of the stethoscope to the abdomen of pregnant females, the maternal pulse was often increased; and if so, the condition of the fœtal circulation might be influenced in such a way as to mar the inferences arrived at by Mr Naylor's well-intended observations. Mr Naylor had done good service, and such endeavours on the part of our assistants should always be encouraged.

Dr Ritchie made a few observations.

VII. NOTE ON SEVERE POST-ABORTION HÆMORRHAGE, CHECKED BY HAMAMELIS VIRGINICA, by Dr Hewan of London.

The President thanked Dr Hewan for his communication.

Dr Keiller would not venture to say that the "Hamamelis Virginica" could not and did not check the severity of the post-abortion hæmorrhage in the case referred to; but from what he knew of such cases, and of what nature could do and did for them, he was getting more and more convinced that it is by no means easy in many cases to determine how far the special means used may have influenced the results. He (Dr K.) cited a very illustrative case which he had that day been called to.

After some remarks from *Dr Craig*, public business was concluded.

SESSION XXXV.—MEETING V.

26th January.—Professor SIMPSON, *President, in the Chair.*

I. *Dr Kirk Duncanson* showed THREE URINARY CALCULI, which he had removed from an elderly female with the aid of a urethral dilator.

II. *Dr Matthews Duncan* then read a paper, entitled REMARKS ON THE INEVITABLE AND OTHER LACERATIONS OF THE ORIFICE OF THE VAGINA, AND NEAR IT, IN PRIMIPARÆ, which appears at page 822.

Dr Underhill had lately seen two cases in primiparæ in which the vaginal orifice had been lacerated at its posterior part, forming a pouch in which the discharges collected, while the perineum remained, in one case almost, in the other altogether, entire. This necessitated the use of antiseptic injections, otherwise the retention of the discharges might have been attended with disastrous results.

Dr Macrae, in the course of fifteen years' practice, had only met with one case of perineal rupture. In his experience, vaginal rupture was not frequent. He found digital dilatation and pressure on the head during its progress the best means for preventing such accidents.

Dr Gordon remarked, that the age of the patient might have some effect in causing rupture, the tissues of elderly females being, in his opinion, rigid and easily lacerable.

Dr Wilson thought Dr Duncan exaggerated the frequency with which these lacerations occurred.

Professor Simpson had listened with great pleasure to Dr Duncan's paper, and the very clear description he gave of the external generative parts. It was usually easy in the virgin, or in a woman who had not given birth to a child, to determine the exact relation of the vaginal orifice within the perineal margin. But after a labour at term, the anatomical limit of the orifice was not so readily made out. For, in addition to the lacerations of the hymeneal crescents on which Dr Duncan had laid stress, and the constancy of which in first labours had been pointed out by Bellien and others, there frequently took place a fissuring of the fourchette, which left the vaginal orifice freely exposed at its posterior margin. In these cases, the determination of the anatomical orifice of the vagina might be made, not only from the situation of the Bartholinian glands and carunculæ myrtiformes, but from the margin of the constrictor vaginæ.

Dr Duncan thanked the Society for the manner in which his paper had been received. In regard to the statement made by Dr Gordon as to the increased liability to laceration in elderly females, he regarded it as a mere assertion, which, in his opinion, was incorrect, and totally unproved by any reliable facts or data which had come under his cognisance.

Part Fourth.

PERISCOPE.

On the Presence of Sugar in Healthy Blood. By Dr M. Abeles, Physician in Carlsbad.—This is an important paper on a subject of much interest. It embodies the results of an elaborate investigation, conducted chiefly in the laboratories of Professors Stricker and Ludwig, the experiments on animals being performed under Professor Stricker's superintendence, and the chemical analyses under that of Professor Ludwig.

In beginning the paper the author gives a short account of the literature of the subject. He mentions the detection of sugar in healthy blood by Thompson, Magendie, and C. Schmidt. He then describes the well-known views of Claude Bernard, who regards the liver as the main source of sugar, both as it occurs in small quantity in the healthy, and in large quantity in the diabetic subject. It will be remembered that, according to Bernard, the liver produces a substance to which he has given the name of glycogen, upon which the blood, as it streams through the organ, acts as a ferment, converting it into grape sugar; and that the sugar thus formed is carried by the blood to the lungs, where it is burnt up, or at least so altered, that in the arterial blood it is no longer detectable as sugar. The diabetic condition, according to Bernard, is brought about by an exaltation of this sugar-producing function of the liver, by virtue of which the quantity eliminated is so great that the lungs are no longer able to cope with it, and large quantities pass unchanged into the arterial circulation. The proofs adduced in support of this doctrine are that, in the normal liver, Bernard found after death not only glycogen but sugar abundantly present; and that, in the blood of the hepatic veins and right chambers of the heart, he found abundant sugar, whereas in the general venous and arterial systems, and in the blood of the portal vein, he could detect only traces of it.

The views of Bernard were almost universally accepted until they were in a very material way corrected by the well-known observations of Pavy. This observer always found in the liver abundance of the so-called glycogen, but in most other respects his conclusions are at variance with those of Bernard. Thus Pavy found no excess of sugar in the blood of the venous system between the liver and the lungs, and when he killed an animal suddenly, and analyzed its liver substance immediately after death, he could detect, along with the abundant glycogen, only minute traces of grape sugar. When, on the other hand, the analysis was not carried out immediately after the death of the animal, he found that sugar developed itself in the dead liver substance with remarkable rapidity. He naturally concludes that in the state of health the blood does not act as a ferment upon the glycogen so as to convert it into sugar. He suggests that such fermentative action is probably prevented in health by some influence from the nervous system, and that in the diabetic condition this influence falls into abeyance, so that the fermentative action of the blood upon the glycogen is unchecked, and becomes as marked as it is after death.

The observations of Pavy have, in a general way, been confirmed by Tscherinoff, Ritter, Meissner, Schiff, and M'Connell. One of the latest works on the subject is that of Bock and Hoffmann (Experimentalstudien über Diabetes, Berlin, 1874). The authors of this work have found that sugar is present in normal blood and in all parts of the circulation, not in mere traces, but in very considerable quantities. In order to determine if the liver is the source of the sugar, they shut off that organ from the general circulation by ligaturing its arterial and portal supply, and obstructing its veins and lymphatics. The animals thus treated lived from 30 to 100 minutes, and if they survived 45 minutes the observers found that sugar vanished entirely from the circulation.

The present author began his investigations by estimating the quantity of sugar normally contained in the blood of various animals, such as rabbits and dogs, and on two occasions he obtained blood by venesection from healthy men. In all of his analyses he found, in agreement with Bock and Hoffmann, sugar present not merely in traces but in considerable quantity—namely, on an average, 0·047 per cent. in the arterial system, and 0·053 in the venous, there being thus, as a general rule, a slight excess in the venous as compared with the arterial system. By a special series of experiments he determined the proportion of sugar in the blood of the vena portæ as compared with that in the blood of the systemic veins and in the right side of the heart. The percentage was as follows: right heart 0·053, vena cava asc. 0·054, v. port. 0·053. The author's observations, like those of Claude Bernard, thus show that sugar is not more abundantly present in the portal vein than in the general circulation.

After estimating the quantity of sugar contained in the different parts of the circulation, the author addressed himself to the great question regarding its source. With regard to this question his observations do not agree with those of Bock and Hoffmann, for whilst the latter observers found that when the liver was shut off from the general circulation the sugar rapidly vanished from the blood, he found that in his hands the experiment had no such effect. In discussing the possible sources of the sugar, the author directs attention to the fact that glycogen is normally present in other parts besides the liver. It is, for example, present in muscular fibre; and although, as Zimmer had previously shown, its proportion in muscle is not so great as in liver substances, yet its sum-total in the muscular system must greatly exceed that in the liver. The author, however, thinks it by no means proved that the normal sugar of the blood proceeds from the glycogen of the liver or other parts. His experiments, agreeing as they do with those of Schiff, Ritter, and others, have led him to the conclusion that the healthy blood exercises no fermentative action upon glycogen; and since he finds in the portal vein and in the general venous system often a greater proportion of sugar than in the blood of the hepatic veins, he believes himself justified in concluding that in the state of health the liver must not be regarded as the source of the natural sugar of the blood. As regards the diabetic condition, he believes that reliable evidence regarding the relation of the liver to the disease will only be obtained when observers, in producing diabetes artificially, select larger animals than rabbits for experimental investigation. He thus concludes that, at present, the source of sugar in the blood, both in health and disease, must be regarded as altogether an open question.—*Medizinische Jahrbücher, redigirt von S. Stricker*, Jahrgang 1875, iii. Heft.

CHARITY HOSPITAL AT BERLIN.—MEDICAL CLINIC. By T. M. ROTCH, M.D.—CASE OF ANÆMIA PERNICIOSA.—14th November 1875, the patient, a man 50 years of age, entered the hospital. He knew nothing of his family history excepting that his parents died of typhoid fever. Previously to his present illness he had felt strong, and had been able to do a hard day's work, that of a street labourer. He had had comfortable lodgings and good food. According to his own statement, he contracted a chancre in 1860, but this was followed by no observable symptoms. In 1870 he had an attack of pneumonia of the left lung, from which he recovered completely, but has since then had an ulcer of the left leg, which has lasted up to the present time, without, however, particularly inconveniencing him. For the last three or four months he has noticed a decided failure in his strength, with loss of appetite. At the time of his entrance he complained of pain extending from the back part of the head, on the left side, around to and across the forehead; pain also in the left eye. He had no appetite; his bowels were rather constipated. He was a man of fair muscular development, but there was great pallor of the lips; small points of hæmorrhage appeared in the skin of the chest and in that of the back of the left hand, and there was the same appearance in the mucous membrane of the gums.

On physical examination nothing abnormal was found in the heart or lungs; there was no enlargement of the spleen or glands; nothing abnormal was detected in the abdomen; small points of hæmorrhage were observed on the retina of left eye. Temperature, 38° Cent.; it has been a little higher. Pulse weak, not increased in frequency; urine normal; specific gravity 1016. The blood, microscopically examined, showed a high grade of anæmia. Professor Traube diagnosticated anæmia perniciosa, whose ætiology was septic poisoning originating from the ulcus cruris. The prognosis was very unfavourable. The treatment consisted of iron, with stimulants, etc.

4th December.—The patient gradually grew weaker, and died this morning.

Autopsy by Professor Virchow.—Heart almost normal, showing only a very slight fatty degeneration of the papillary muscles, and a few hæmorrhagic points in the endocardium. There was sclerosis (beginning atheroma) of the aorta. Hæmorrhagic points appeared in the pleura and diaphragm. The lungs, spleen, liver, stomach, and right kidney were normal; the left kidney was normal, excepting in one pyramid, which showed diphtheritic changes. There was very extensive diphtheritis of the ileum and large intestine. Hæmorrhages were found in the dura mater aud pia mater, and in the periphery of the cerebellum, on the left side, in the gray substance; some hæmorrhagic points were also observed in the neighbouring white substance, which showed yellow œdema. The other parts of the brain were normal. The blood microscopically showed great hydræmia.

Professor Virchow made the anatomical diagnosis of anæmia perniciosa and diphtheritis of the intestine and kidney. He said in regard to the ætiology, "The disease probably has nothing to do with the ulcus cruris. Nothing as yet has been proved in regard to its origin. I am inclined to believe that it is malarial."

The interesting points of this case were, *first*, that the heart was not decidedly fatty, which is usually the case in anæmia perniciosa; *second*, the existence of an extensive diphtheritis of the intestine, with no symptoms of the same during life. So that pathologically it was not a pure case of what is usually recognised as anæmia perniciosa, although clinically it presented the symptoms of that disease.—*Boston Med. and Surg. Journal, 27th Jan.*

"DEATH FROM ETHER."—We abridge the following statement from the *New York Herald* of 22d November. A man, fifty-four years of age, went to the homœopathic college in New York, on Saturday the 20th inst. "He was complaining of pain in the left upper jaw extending to the head, with great nervous prostration. There were four openings in the jaw, all discharging fœtid pus. On introducing a probe, caries of the upper jaw was found, and the patient was advised to have the bone removed. He was placed under the influence of ether and *laid on the table*" (the italics are ours). "A physician was constantly feeling the patient's pulse while the ether was being given." An "incision was made into the upper jaw, and four teeth extracted. The operator was "about to extend the incision when he noticed the face of the patient become blue. Artificial respiration and the galvanic battery were applied, but they were unavailing; he was dead." The reputation of ether, in doses even of a pound or two, has been hitherto pretty good. It has been tried on patients with hearts variously fatty. It will be difficult to persuade us that two and one-fourth ounces of this hitherto beneficent agent has proved so suddenly fatal, without any previous gradual diminution of the pulse. It should be conclusively shown that the patient died of nothing else. *This patient, so far as we can judge, died of asphyxia from blood in the trachea.* In the first place, after he was etherized, he was laid on his back. An "incision" was made in the jaw, and time enough elapsed to extract four teeth. Dead bone always makes the soft parts vascular. There was ample time for blood to fill the trachea. Until it is shown that there was no blood in the fauces and trachea, this must stand as the cause of death. The autopsy is silent on this point. The suddenness and the lividity both belong to asphyxia, and neither to etherization. In the Massachusetts General Hospital, operations on the mouth are usually done in the erect posture, with special attention to keeping the fauces clear of blood.—*Boston Medical and Surgical Journal*, 25th November 1875.

THE following, recommended by M. Laborde as a new and very advantageous method of preparing raw meat, is given in the *Lyon Médical*:—A not very thick broth of tapioca is first prepared, and is then allowed to cool, so that it may not cook the meat with which it is subsequently mixed. The meat, having been finely scraped, is diluted with a quantity of cold soup until the mixture is thoroughly made and has the appearance and consistency of a tomato soup. Thus prepared, it only remains to turn in, little by little, the tapioca porridge, stirring it constantly, and there is obtained a perfectly homogeneous broth, in which, when properly made, the meat is so well disguised that the person who eats it, unless previously informed, does not discover it. Under the name of "medicinal porridge of tapioca," raw meat has thus been administered to patients, and has been taken by them with great relish.—*Boston Medical and Surgical Journal*, 21st October 1875.

SIR HENRY THOMPSON ON MEDDLESOME URETHRAL SURGERY.—I may be allowed to say, perhaps, that my feeble voice has also been raised against the abuse of instruments from the first day that I ventured to pen a line on the subject, some two or three and twenty years ago. And now I perceive a growing disposition to return to the state of things I have referred to. I note an increased tendency to discover stricture, and especially to undertake a considerable amount of operative treatment for strictures of the slightest kind, and sometimes where, in my opinion, they do not exist. There seems now to be a school which has determined for itself a very high standard of patency in what we hear called the "urethral tube," and which is accordingly said to have, or, if it hasn't, that it ought to have, a calibre of so many parts, and very large parts, of an inch, or millimetres, as the case may be. Instruments of astounding magnitude are produced, and if one of them cannot be drawn, with an ease which contents the operator, through the whole of the urethra, the unlucky patient is pronounced to be the subject of stricture; and probably he is submitted to an operation by no means devoid of risk. Now, I don't know that this fashion has as yet been adopted here, but I do know that it exists elsewhere, and I raise my earnest protest against it.—*Lancet*, 11th Dec. 1875.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. HANDSEL GRIFFITHS, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal*.]

CHLORAL.—(*a.*) *Physiological Action.*—Rokitansky affirms that chloral is a direct heart poison, but only when concentrated; it reduces the irritability of the respiratory centres, and also of the vasomotor centres.—*Stricker's Jahrbücher*, 1875.

Musculus and De Merme have discovered a curious acid in the urine after the ingestion of chloral hydrate. It is precipitated by basic acetate of lead, and forms stellate crystals like those of tyrosine. It decomposes carbonates with effervescence, and salts of it are not affected by acetic acid. It is soluble in water and alcohol, but not in ether. Salts of copper, silver, and bismuth are reduced by it, and it decolorizes sulphate of indigo. All its salts, except the basic lead salt, are soluble in water, but not in absolute alcohol. The discoverers propose to call it urochloralic acid.—*Comptes Rendus*, 1875.

(*b*.) *As a Surgical Dressing.*—Prof. Marc Sée strongly advocates the employment of a one-per-cent. aqueous solution of chloral as a surgical dressing. Whenever fœtidity has to be destroyed, fermentation, putrefaction, or the production of vibriones, etc., to be arrested, it fulfils the indication with certainty and inoffensiveness. —*Jl. de Thérapeutique*, 25th July 1875.

Mr Lucas, of Guy's Hospital, has given chloral an extensive trial as an application to sloughing wounds and fœtid ulcers. The effect of the local application appears to be that of a powerful stimulant and disinfectant; it has no soothing or sedative effect on the part to which it is applied, but gives rise to considerable pain, which lasts some time. It never becomes absorbed in sufficient quantity to act as a hypnotic, and its local application is therefore eminently safe and free from the dangers which sometimes follow the use of carbolic lotion long continued. He has found a solution of four grains of chloral in an ounce of water to be the most useful.—*Lancet*, 16th Oct. 1875.

Dr C. A. Peabody recommends a solution of from three to five grains of chloral in an ounce of water, as a substitute for carbolic acid for external use. It acts promptly in ulcers. In cases of amputation, where the surfaces took on an unhealthy action, the application of the solution changed the character within twenty-four hours. It removes fœtor, stimulates granulation, is clean, and does not stain.—*Canada Medical Record*, 1875.

In the *Edinburgh Medical Journal*, Feb. 1876, is a paper by Dr William Craig, on the value of solution of chloral as a preservative fluid for anatomical specimens, and as a dressing for wounds and ulcers.

(*c*.) *In Fissure of the Anus.*—Créquy treats fissure of the anus by chloral. His procedure is as follows: Charpie, soaked in a two-per-cent. solution of chloral, is inserted just within the anus, daily attention being of course duly paid to the regularity of the bowels. In the two cases recorded as having been so treated, a cure was effected within a fortnight.

(*d*.) *In Cerebral Rheumatism.*—M. Bouchut records three cases of cerebral rheumatism which he treated successfully by chloral hydrate. He gave from forty to eighty grains in one or two doses closely repeated.—*Bull. Gen. de Thér.*, 12th Nov. 1875.

(*e*.) *In Sea-Sickness.*—Dr Obet finds that chloral is by far the best remedy for sea-sickness. It was first recommended for this affection by Dr Giraldes. On the first day of the voyage, sleep should be at once secured by administering fifteen grains of the chloral at a single dose, and the following days twenty or thirty grains may be given during the day in small hourly doses. As a general rule, two or three days will suffice to accustom passengers thus treated to the sea.—*Archiv de Med. Navale*, June 1875.

(*f*.) *In Cholera.*—Surgeon A. R. Hall, of the U. S. Army Medical Department, treats cholera in the cold stage by hypodermic injections of chloral. He uses a solution of 10 grains dissolved in 100 drops of water, and of this he injects from 10 to 20 minims in the arms. Beyond a slight pain and hardness at the point of puncture, there was no local evidence, twelve hours afterwards, of the injection having been made. Improvement of the temperature of the surface, and in the general appearance, abolition of cramps, and resumption of the urinary secretion, followed the adoption of this treatment in every case. Only one of fourteen cases died, and in this one the treatment was not carried out efficiently. Chloral hydrate, Mr Hall says, acts as a sedative to the vaso-motor system, and thus relieves the spasm of the involuntary muscular fibre, and brings the stage of collapse to an end.—*New Rem.*, Oct. 1875.

(*g.*) *In Labour.*—Dr C. A. Prentiss states that the following are the effects of chloral in labour:—It conduces to a quiet and tranquil mind; abridges the duration and modifies the severity of each pain; promotes pleasant and refreshing sleep in the intervals of pain; relaxes the os, and so abridges the duration of labour. He gives 5 grains every fifteen minutes in the first stage, until 20 or 30 grains have been given. In four or six hours, more may be given, if the labour continues and the remedy be indicated. He considers the induction of anæsthesia by chloral to be unsafe and unnecessary.—*Hay's American Journal of Medical Science*, Jan. 1876.

(*h.*) *In Puerperal Convulsions.*—Portal records three cases of puerperal convulsions successfully treated by chloral. They were all subjects of albuminuria. 90 grains of chloral were given in each case.—*Bull. Gen. de Thérapeutique,* 15th August 1875.

(*i.*) *In Cancer of the Uterus.*—Dr Fleischer uses chloral locally in carcinoma uteri. He first washes out the vagina thoroughly, and then passes up to the cancerous surface some cotton-wool, wetted with a solution of chloral (2 drachms to 3 ounces of water); this application is repeated every two hours. After a few applications the pain is moderated, and the discharge becomes less offensive.—*Med. Chir. Centralblatt*, ix., 1875.

(*j.*) *As an Anæsthetic for Children.*—M. Bouchut stated at the Brussels Congress that he obtains anæsthesia in children with from 45 to 60 grains of chloral at one dose. The child soon goes to sleep, and anæsthesia is complete in about one hour. No such result can be obtained with adults.—*Bull. Gen. de Thérapeutique*, 30th October 1875.

(*k.*) *Strychnine as an Antidote.*—A case of poisoning by chloral is recorded in the *Centralblatt f. d. Med. Wissench.*, 3d April 1875. A man had taken about 370 grains. Artificial respiration and faradization proving useless, two injections subcutaneously, one of 0·003 grammes of strychnine, and a second of 0·002 grammes, were made. The result of this treatment was, that thirty-two hours after the poisoning, he awoke fresh and free from all ill effects. The fact that the stomach was full when the poison was taken would account for the circumstance that no gastritis resulted.

In the *Pharmaceutical Journal*, 21st and 28th August 1875, are communications from Mr J. Gilbert Candy and Mr W. S. Greaves, in which they record experiments on dogs which afford evidence of the antidotal power of chloral over strychnine.

ERGOT.—(*a.*) *A New Alkaloid from Ergot.*—M. C. Tanret details a process by which he has succeeded in extracting a new alkaloid, which he terms ergotinine. It exists in ergot in very small quantity, and is extremely unstable. Its most characteristic reaction is its coloration with moderately concentrated sulphuric acid; the colour is at first a yellowish red, and ultimately becomes an intense violet blue. Ergotinine is strongly alkaline, and capable of saturating acids, forming salts, which quickly become red on exposure to air. —*Comptes Rendus*, vol. lxxxi. p. 896.

(*b.*) *Effects of the Aqueous and Alcoholic Solutions of Ergot.*—The Committee on Therapeutics of the Chicago Society of Physicians and Surgeons report as follows respecting the aqueous and alcoholic solutions of ergot:—They differ in their effects on the circulatory system. The aqueous solution excites the activity of the cardiac inhibitory centres, and the vaso-motor centre in the medulla; it slackens the pulse, narrows the calibre of the small arteries, and increases the blood-pressure. Very large doses paralyze the heart at once, and so effectually that the induced galvanic current fails to restore it to action. These effects are not produced by the alcoholic solution. The latter produces symptoms akin to those of acro-narcotic poisoning—viz., irritation of the mucous membrane of the stomach and bowels, tonic cramps, and agitative violent spasms.—*New Remedies*, 1875.

(*c.*) *Effect on the Mammary Secretion.*—Dr J. Sehtscherbinkenhoff observed that, during an epidemic of ergot poisoning, there was frequently a cessation of the flow of milk in nursing women as soon as the symptoms of ergotism appeared. He was thus led to administer ergot in cases of accumulation of milk in the breasts, and even in actual inflammation of the glands. His results were perfectly successful. He found that, at the time of weaning, ergot caused a speedy cessation of the lacteal secretion.—*Centralblatt für Chirurg.*, 8th May 1875.

(*d.*) *In Acute Mania.*—Dr Van Andel uses, in acute mania, hypodermic injections of ergotin dissolved in alcohol and glycerine. He believes that it acts beneficially by contracting the cerebral vessels. —*New Remedies*, July 1875.

(*e.*) *In Enteric Fever.*—M. Hayem finds that, in enteric fever, ergot acts satisfactorily as an antipyretic. Under the influence of this drug, he finds that the defervescence is much more rapid than when sulphate of quinine or digitalis is used. The dose administered varied from 30 to 50 grains in the twenty-four hours.—*Rev. de Thér.*, 1st October.

(*f.*) *In Pneumonia.*—Dr Wycisk was led to employ ergot in cases of croupous pneumonia on account of the contractile influence exerted by that drug on the vessels. The effect of the ergot in all cases was to diminish the amount of the exudation. In large infiltrations, in emphysema, or in exhausted or decrepit individuals, the employment of ergot might prove hazardous.—*Allg. Med. Central Zeitung.*

(*g.*) *In Diabetes Insipidus.* — Prof. Sydney Ringer, in the *British Medical Journal*, 25th December 1875, records a well-marked case of diabetes insipidus which was uninfluenced by jaborandi, whilst on three occasions the quantity of urine was very greatly reduced by ergot.

(*h.*) *In Uterine Fibroids.*—Hildebrand states that the treatment of uterine fibroids by hypodermic injection of ergot is most likely to be successful:—1. When the tumour is richly provided with muscular tissue, and possesses the consistence and feel of a tense elastic cyst. 2. When the tumour is submucous. 3. When the walls of the uterus are sound, capable of vigorous contraction, not too much attenuated by dilatation, or stiffened by exudation in their substance, and when there is no para- or peri- metritis present. 4. As soon as the chronic metritis and parametritis, which often accompany fibroid tumours, have been removed by proper preparatory treatment, when the previously-mentioned conditions again come into force. 5. When the tumour is unprovided with a capsule, and merges directly without a boundary into the peculiar tissue of the uterus.—*American Journal of Obstetrics*, Feb. 1875.

(*i.*) *In Cystic Paralysis.*—Dr J. Williamsky determined experimentally that in dogs and rabbits, ergot causes immediate temporary contraction of the bladder. He consequently employed it in the form of injections of the fluid extract in two cases of cystic paralysis, and with very successful results.—*Clinic*, May 1875.

(*j.*) *In Internal Hæmorrhoids.*—Dr Orr reports that he treats internal hæmorrhoids very successfully by rectal injections daily of half a drachm of the fluid extract of ergot, dissolved in an ounce of water.—*Clinic*, April 1875.

(*k.*) *In Epistaxis.*—Dr George St George, of Lisburn, states that he has found the internal administration of ergot of great value in the treatment of epistaxis. He generally gives the liquid extract in fifteen-drop doses every quarter of an hour until the hæmorrhage ceases.—*British Medical Journal*, 1st January 1876.

GELSEMIUM. — (*a.*) *Physiological Action.* — Drs Ringer and Murrell prove experimentally that paralysis of the reflex and voluntary power is not due to the action of the drug on the muscles or on the motor nerves, but that by its direct action on the spinal cord it destroys reflex power, and, probably by its influence on the cord, loss of voluntary power is produced. They conclude that gelsemium probably exerts no influence on the motor nerves. While testing the effects of large doses of this drug on the heart, they found that it produced well-marked tetanus. The tetanus induced by strychnia differs from that resulting from gelsemium in the following particulars:—1. Strychnia tetanus supervenes without previous loss of voluntary or reflex power. 2. Respiration continues unimpaired during the paroxysms. 3. Every part of the body is affected, the anterior extremities as strongly as the posterior. 4. The cord is much less rapidly exhausted; thus a fresh paroxysm could be excited directly the previous one had declined, and in this way it could be reproduced many times without any interval or apparent diminution of intensity. 5. Strychnia tetanus persists for many hours, or even for several days.

Gelsemium tetanus, on the other hand—1. Is always preceded by considerable loss of voluntary and reflex power. 2. Respiration ceases before the onset of the convulsion. 3. The posterior extremities are most affected. 4. Irritation fails to excite another paroxysm till the lapse of some seconds, as if the exhausted cord required time to renew its energy. 5. It lasts only a short time; sometimes only half-an-hour, rarely more than three hours.

The authors infer that the tetanic convulsions are due to the action of gelsemium on the spinal cord. They draw particular attention to the curious fact that the tetanus is always preceded by considerable loss of voluntary and reflex power. The question arises, Does the plant contain a single active principle which first paralyzes and then tetanizes, or does it contain two agents, one capable of paralyzing, the other of exciting the cord? The authors incline to the latter view. In Mr Gerrard's preparation of the alkaloid, the tetanizing agent exists in larger proportion, whilst the paralyzing agent predominates in the liquid extract. The tetanizing agent produces its effects rather more tardily than the paralyzing agent. Thus, in employing the same proportion of each preparation of identical strength, a small dose of the alkaloid will exert less paralyzing effect than the equivalent dose of liquid extract; whilst, with a large dose of the alkaloid, the tetanizing principle is able to overcome the depression of the cord produced by the paralyzing principle; but as the action of the tetanizing agent comes on later, we get first paralysis and then tetanus. On the other hand, in using the liquid extract when the paralyzing agent is in excess of the tetanizing ingredient, small doses paralyze more completely than the corresponding quantity of the alkaloid solution; whilst large doses of the liquid extract, like fifty minims, produce paralysis so profound that the tetanizing ingredient fails to produce its characteristic effect.

As gelsemium so profoundly depresses the functions of the cord, the authors thought that it would prevent or arrest the tetanic convulsions produced by strychnia. They experimentally determined that such was the case.—*Lancet*, 25th Dec. 1875, and 15th and 22d Jan. 1876.

Dr J. Ott gives the following summary of the physiological effects of gelsemia :—1. In cold-blooded animals, it paralyzes, first the sensory ganglia, and then the motor ganglia, in the central nervous system. This order is reversed in warm-blooded animals. 2. It diminishes the pulse and blood-pressure. 3. The decrease in the pulse rate is due to lessened irritability of the excito-motor ganglia of the heart. 4. The fall of pressure is due to diminution of cardiac irritability and vaso-motor tonus. 5. It decreases the respiration through a paralyzing action on the respiratory centres. 6. It dilates the pupils. 7. It reduces the temperature.—*Phil. Med. and Surg. Reporter*, 11th Dec. 1875.

(*b.*) *As an Antineuralgic.*—Dr A. Jurasz of Heidelberg speaks highly in favour of gelsemium as an antineuralgic. In one case, which was that of a man who suffered from neuralgia of the first branch of the fifth nerve on the right side, and in which quinine, given internally, and veratria ointment applied externally, failed to afford relief, the administration of five-drop doses of the tincture every eight hours procured perfect and permanent relief in the course of three days. In a case of brachial neuralgia, one of sciatica, and in two cases of trigeminal neuralgia, it yielded satisfactory results. It failed, however, in a case of hemicrania of long duration, and in two cases of muscular rheumatism.—*Centralblatt f. d. Med. Wiss.*, No. 31, 1875.

In a paper by Dr Spencer Thomson, in the *Lancet* for 6th Nov. 1875, on the rapid relief of neuralgic pain, he states that he has used gelsemium in at least forty cases with almost constant success. His experience is, that the remedial power of the drug seems confined to those branches of the trifacial nerve supplying the upper and lower jaw, more particularly the latter, and more especially when the pain is referred to the teeth or alveoli. Gelsemium exercises no effect whatever on frontal pain.

In the *Practitioner* for August 1875, Dr James Sawyer writes that he has rarely found gelsemium fail to give decided and lasting relief in cases of neuralgic pains in the face and jaws associated with carious teeth. He usually gives 15 minims of a tincture prepared as follows :—Take of gelsemium root coarsely powdered 2 oz., of rectified spirit 20 oz. Moisten the root with 10 oz. of the spirit, and allow the mixture to stand for twenty-four hours. At the end of that time, pack in a percolator, and add the remaining 10 oz. of spirit. When the fluid has ceased to flow, remove the contents of the percolator and press them. Add the pressed liquid to that obtained by percolation, filter, and make up with rectified spirit to a pint. Eleven minims of this tincture are equal to about one grain of the root.

(*c.*) *For the Relief of Cough.*—Dr J. Roberts Thomson advocates the employment of tincture of gelsemium for the relief of cough. In the cases enumerated by him, he usually gives it in five-minim doses, with the effect of affording great relief. In cases characterized by much bronchial irritation, the gelsemium may be advantageously combined with bromide of ammonium, tincture of squill, syrup of codeia, etc.—*Brit. Med. Jour.*, 16th Oct. 1875.

In the *Pharmaceutical Journal*, 18th Dec. 1875, and 1st, 15th, and 29th Jan. 1876, is an exhaustive *résumé* by Mr Holmes of what has hitherto been written concerning the history, botanical characters, medicinal properties and uses, medicinal preparations, dosage, and chemical composition of gelsemium.

Part Fifth.

MEDICAL NEWS.

LIST of ARMY MEDICAL CANDIDATES who were successful at both the London and Netley Examinations, having passed through a course of instruction at the Army Medical School, Netley, February 1876.

	No. of Marks.		No. of Marks.
1. Hewett, F. C. C.	5012	8. Fleming, H. B.	3520
2. Le Grand, W. J.	4592	9. Daubeny, C. A.	3433
3. Taaffe, R.	4275	10. Burges, W. A.	3356
4. Findlay, J.	3894	11. Ievers, P. G.	3030
5. Briggs, W. H.	3818	12. Hayes, P. A.	2995
6. Thompson, T. W.	3771	13. James, W. M.	2933
7. Fenn, E. H.	3537	14. Kirkwood, K. D. L.	2565

LIST of INDIAN MEDICAL CANDIDATES who were successful at both the London and Netley Examinations, having passed through a course of instruction at the Army Medical School, Netley, February 1876.

	No. of Marks.		No. of Marks.
* 1. Weir, P. A.	5536	6. Pedroza, F. H.	4681
2. Freyer, P. J.	5508	7. O'Neill, J.	4675
3. Haig, P. de H.	4826	8. Clarkson, J. W.	4472
4. Lewtas, J.	4729	9. Parker, Jos.	4188
5. Ferrand, E.	4708	10. Day, C. H.	4096

* Gained the Herbert Prize.

OBITUARY.

THE LATE JOSEPH R. HARDIE, M.B., ETC.—The sudden and quite unexpected death of this rising young physician has caused much interest and sorrow among his numerous friends. He had only reached the age of thirty, having graduated in the University of Edinburgh in 1868. Yet he was widely known and highly esteemed. His simple, pure, and open character, joined to a fine manly physical frame, was of itself a great natural recommendation. It was only those who were intimate with him who knew the great excellence of his character, his unassuming piety, his rare integrity in word and deed, his humility, his kindness.

After he graduated, he spent some time as resident physician in an asylum for insane in England, and we believe also as a *locum tenens* for an English practitioner. But in 1870 he became connected, as assistant, with Dr Matthews Duncan, and this fortunate and happy alliance continued till his premature and regretted death.

He was the author of three good papers, which were published in our pages:—On a Peculiar Polypus of the Uterus; On the Cause of Retention of Urine in Cases of Fibrous Tumour of the Uterus; On a Method of Pelvimetry. And he discovered the prickle epithelium on the mucous membrane covering a uterine fibroid.

On Thursday, the 10th February, he was ailing, yet he visited several patients. He was, however, alarmed about his health, thinking something was going wrong in his brain. On Friday morning, his mother, entering his room, found him sleeping. Subsequently, getting alarmed at his delay in coming to breakfast, she revisited his chamber, and found he was dead.

A post-mortem examination revealed great congestion of the brain.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

SCROFULOUS SORES OR RUNNINGS.

FREE CHURCH MANSE, CROSSHILL, BY MAYBOLE,
14*th February* 1876.

MR EDITOR,—For the last twelve or thirteen years I have been prescribing the following remedy for scrofulous sores or runnings. It consists of three grains of corrosive sublimate in a pint bottle of whisky; a rag dipped in this twice or thrice a day, and laid on the sores, and continued till all the sores are dried up, is all that is needed.

Not one single case, in which it got fair play, ever failed of a cure. In one person the shoulder-blade was decaying. The pain was so great that the patient could not work. It was pronounced by a very skilful surgeon to be hopeless, and only a matter of time. In a few weeks this man was able to work, and works still, though years have passed.

Another case was where the patient had suffered for many years. Had been at many skilled surgeons far and near. Had to walk with stilts about the house. The pain at night was so great, she often spent much of the night weeping, as she told me. After a short trial of the above remedy, she was able to lay aside her stilts, and walk with a staff. She has been able, for the last seven or eight years, to walk three miles—sometimes seven miles—on a stretch, without either staff or stilt.

I could easily multiply the cases; but let these suffice.

Looking on it as a boon to suffering humanity, I do not think it right, after having so fully tested it, to keep the secret any longer. I would implore surgeons to give it a fair trial wherever they have opportunity. Let them communicate their experience to your columns after a few months. If they do so, I am not afraid of the result.—Yours, etc., JOHN M'LENNAN.

PERIODICALS RECEIVED.

American Clinical Lectures,—Nos. 3, 11.
American Journal of Insanity,—July, Oct., Jan.
American Journal of the Medical Sciences,—July, Oct., Jan.
American Journal of Obstetrics,—August, Nov.
American Practitioner,—July to Dec.
Anales de Ciencias Médicas,—Jan. 10 to Feb. 10.
Annales d'Oculistique,—July to Dec.
Annali Universali di Medicina e Chirurgia,—July to Jan.
Archives Générales de Médecine,—August to Feb.
Archives de Physiologie,—Aug. to Dec.
Archives de Tocologie,—Aug. to Feb.
Archives of Dermatology,—July, Oct., Jan.; Vol. for 1874-75.
Berliner Klin. Wochenschrift,—July 26 to Feb. 21.
Births, Deaths, and Marriages, Monthly Return of,—July to Jan.; Quarterly Return,—June, Sept., Dec.
Boston Medical and Surgical Journal,—July 15 to Feb. 3.
British and Foreign Medico-Chirurgical Review,—October, January.
British Medical Journal,—July 31 to Feb. 19.
Bulletin Général de Thérapeutique,—July 30 to Feb. 15.
Canadian Jour. of Med. Science,—Jan., Feb.
Chicago Journal,—July, Oct., Jan.
Chicago Medical Journal and Examiner,—Jan.
Detroit Review of Medicine,—Aug. to Jan.
Druggist,—July, Aug., Sept., Dec.
Dublin Journal of Medical Science,—Aug. to Feb.
Gazette des Hôpitaux,—July 22 to Feb. 19.
Gazette Hebdomadaire de Médecine,—July 30 to Feb. 19.
Gazette Médicale de Paris,—July 24 to Feb. 19.
Gazzetta Medica delle Calabrie,—Jan.
Glasgow Medical Journal,—Oct., Jan.
Indian Medical Gazette,—July to Jan.
Journal de Thérapeutique,—July 25 to Feb. 10.
Journal d'Hygiène,—Oct. 1.
Journal of Anatomy and Physiology,—Jan.
Journal of Mental Science,—October, January.
Journal of the Scottish Meteorological Society,—July 1874 to July 1875.
Klinische Monatsbläter für Augenheilkunde,—April to Aug.
London Medical Record,—Aug. to Feb.
Medical Examiner,—Jan. 6 to Feb. 17.
Medical Press and Circular,—July 28 to Feb. 16.
Medical Times and Gazette,—July 31 to Feb. 19.
Medizinische Jahrbücher,—Parts 3, 4.
Melbourne Medical Record,—June 19 to July 10, 31; Oct. 15; Nov. 1, 15; Dec. 1.
New Orleans Medical and Surgical Journal,—July, Sept., Nov.
New York Medical Journal,—Aug. to Feb.
Nordiskt Medicinskt Arkiv,—Sjunde Bandet, Andra Häftet, Tredje Häftet.
Obstetrical Journal,—August to Feb.; American Supplement,—July to Jan.
Onderzoekingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschoole,—Derde Reeks, Aflevering 2.
Philadelphia Medical and Surgical Reporter,—July 17 to Feb. 12.
Philadelphia Medical Times,—July 10 to Feb. 5.
Practitioner,—August to Feb.
Progrès Médical,—July 24 to Feb. 19.
Recueil d'Ophthalmologie,—July, Oct., Jan.
Revista Medica de Chile,—June to Nov.
Revue de Thérapeutique Médico-Chirurgicale,—Aug. 1 to Feb. 15.
Richmond and Louisville Medical Journal,—July to Jan.
Sanitarian,—August to Jan.
Sanitary Record,—July 31 to Feb. 19.
Transactions of the Medical Society of the District of Columbia,—July, Oct., Jan.
Upsala Läkareforenings Förhandlingar,—Nos. 1, 2.
Veterinary Journal,—August.
Virchow's Archiv,—July 2 to Dec. 27.
Virginia Medical Monthly,—Dec.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On Rupture of the Perineum, and especially on the Division of the Perineum and Rectovaginal Septum, by Post-Partum Sloughing.* By J. MATTHEWS DUNCAN, M.D.

(*Read before the Obstetrical Society of Edinburgh, 9th February.*)

LACERATION of the perineum, or tearing of it, is the proper designation of the accident that so frequently befalls it during the second stage of labour. In the great majority of cases the laceration is begun by over-distension of the anterior border of this part, which consequently gives way at the fourchette, its weakest portion. The tear, once commenced, is easily made to run backwards to a variable extent through parts which would have maintained their integrity if they had not been taken at a disadvantage, produced by the aforesaid giving way of the fourchette.

Central laceration of the perineum is likewise commenced by over-distension, and subsequently increased. Of this rare accident I have met three examples. In one the rupture was healed by the timely early stitches of the medical attendant. In the other two I operated long after the occurrence of the accident. Both were easily healed.

Laceration of the rectovaginal septum is, so far as I know, regarded as a result, not invariably, but nearly so, of an extension of laceration of the perineum. Of this accident I shall mention two cases where it certainly was not so, for in both a rectovaginal laceration, or rectovaginal fistula, was produced, while the sphincter ani and a considerable extent of perineum remained entire.

Mrs ——, mother of one child, was sent to me from a distant part of Scotland, to be cured of a rectovaginal fistula. The opening was rounded, as large as would transmit a thumb, and its lower margin was just above the sphincter ani. The only account I could get of it was that it was the result of labour, being produced during the second stage. Besides the rectovaginal fistula, the perineum had been extensively injured in the usual way. The woman and her child were healthy. With the aid of Drs Hardie and Underhill I pared the edges of the fistula, and united them by interrupted silver sutures. The woman soon returned home quite cured.

Mrs —— was delivered in 1861 of her first child, and the perineum was, in the usual way, lacerated very extensively; the sphincter ani and rectovaginal septum being involved in the injury. She then had six additional confinements. Her health now became very bad. She had frequent recurrences of diarrhœa, and I operated to restore the perineum in 1872, with complete success. In 1875 I attended her in her eighth confinement. When the head had nearly completely passed the outlet of the ligamentous pelvis, and was distending the perineum as much as it appeared to admit of, I delivered in the absence of a pain, pressing out the head[1] over the thin and still entire perineum as restored in 1872. This pressing out was effected without passing the finger into the rectum.

To this manœuvre I attach importance, only because it was done in the absence of a pain, which might have precipitated the birth; and, on account of the direction of its force, would have produced at least a tendency to laceration in spite of careful support of the weak structures. The delivery was effected without any apparent damage to the restored perineum, except a little laceration of the fourchette. But the discharge of blood and air per anum during the third stage of labour made it plain that the rectovaginal septum had not escaped without injury. Combined rectal and vaginal examination showed that this septum had yielded, under over-distension, at the highest part of the cicatrix, and about 1½ inch above the margin of the anus. The injury thus produced was cured by spontaneous reunion. The laceration in this case, and inferentially in the same woman's first labour, was manifestly the result of want of capacity or of undilatability of the vagina.

The class of cases to which I wish, in this article, to draw particular attention differs from any of the kinds to which I have made reference. The parts in this class of cases are entire after complete delivery, it may be for days; and then they give way, conditions being produced that are apparently identical with those that result from direct laceration during the second stage of labour.

In the first case of this kind which I shall narrate, the course of events is very evident. Over-distension of the centre of the perineum had taken place during delivery to such an extent as was inconsistent with continued life of the most stretched parts, but not with temporary entirety. The result was what has been called a perineal fistula. The necrosis of the persistent entire skin of the centre of the perineum led to the same condition as results from central rupture; only the perineal fistula was a secondary result of the distension, not primary.

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[1] See Edinburgh Medical Journal, January 1875, p. 644. See also some remarks on the old or rectal method, by Dr P. Mundé, American Journal of Obstetrics, November 1875, p. 536.

Mrs ——, a fine healthy young woman, was delivered on 29th September 1873 of a fine healthy male child. The child's head was long arrested in its progress by the perineum. Assisted by Dr Underhill, I applied the forceps and easily effected delivery. The perineum was not lacerated in the usual way, the fourchette even remaining entire. After delivery, there was observed a spot of about three-quarters of an inch in diameter, and slightly livid and prominent, midway between the anus and fourchette, which called for investigation. It was found by simultaneous external and vaginal examination that the tissues of the vagina were deeply lacerated above the prominent part of the perineum, and that the persistent textures, including the skin, were very thin at this point.

Particular attention was paid to the discharges, which always appeared to be healthy and without fœtor. The parts were not visually examined till the ninth day after delivery, when the nurse called my attention to the state of the perineum. I then found that in the middle of the previously swollen part of the perineum there was a rounded opening sufficient to transmit a small cedar pencil into the vagina, or fully one-eighth of an inch in diameter. It healed up before death. On the twelfth day after delivery, peculiar pyæmic symptoms manifested themselves, and a fatal issue ensued on the twenty-seventh day after delivery.

The following case is much more remarkable, as the injury occurring after delivery was much greater, and is much more difficult to account for. For several days after parturition there were the appearances of an ordinary severe laceration, not involving the sphincter ani; and subsequently, without any discoverable additional cause, there was complete division of the perineum, sphincter, and lower part of rectovaginal septum. I extract the details about to be given chiefly from the report by Mr Naylor, house-surgeon, in the hospital case-book. These details are pretty fully given, but there would, of course, have been still more of them had the accident which occurred been foreseen. Those details which have no bearing on the accident are here omitted entirely.

R. C., æt. 28, primipara, is an ill-made woman, of about 4 feet 11 inches in height. She has a contracted rickety pelvis, with projecting sacrum and otherwise reniform brim, whose conjugate is scarcely 2½ inches. She had been about thirty-six hours in labour when delivery was completed. The waters had been discharged about a day before labour began. After thirty-one hours of regular pains, the os uteri was very high, and not larger than easily to admit the finger, though quite soft, and the head was still high above the brim. The cervix was dilated by indiarubber bags for about four hours, and then delivery was effected by version and podalic extraction, the head being perforated when its base was brought to the brim of the pelvis. No difficulty was experienced in completing delivery after the base of the skull had passed the brim. The perineum was lacerated, but as there was nothing apparently peculiar about the accident, the part was not particularly examined at this time. The child weighed 6 lbs. 10 oz.

9*th October.*—The day following delivery. Pulse 96, temp. 99°. Complains of severe pain in the region of the external genitals. The perineum found to be ruptured. A linear fissure of the skin only extended to the verge of the anus, whose sphincter was found to be entire by passing the finger through it. This fissure through the skin (or split skin) was half an inch in extent. The depth of this linear fissure is very little, probably not through the corium, for it is not increased by separating the labia and adjacent parts. When the labia were separated the vaginal structures were seen to be entire for at least one quarter of an inch farther forwards than the entire part of perineum, including the split skin. The wound is healthy, but around it and in the labia there is redness, such as is seen over diffuse cellular inflammation. The urine has to be drawn off; reaction acid; is albuminous.

10*th October.*—Slight jaundice and vomiting. Pulse 96, temp. 98·8°. Bowels moved by medicine. Urine has to be drawn off. State of perineum as before.

11*th October.*—Pulse 92, temp. 98°. Urine has to be drawn off; lithates copious; no albumen. Perineum less inflamed.

12*th October.*—Jaundice diminishing. No vomiting. Pulse 92, temp. 97·8°. Urine has to be drawn off. Perineum has lost its swelling and diffused redness. Cutaneous fissure appears to be as before.

13*th October.*—Is comfortable to-day. Pulse 84, temp. 99°. Urine has to be drawn off. Perineum not examined.

14*th October.*—Pulse 92, temp. 100·6°. Urine has to be drawn off. Perineum not examined.

15*th October.*—Pulse 88, temp. 98·8°. Urine has to be drawn off. Perineal laceration as before. Wound granulating. Anus found entire.

16*th October.*—Pulse 100, temp. 102·5°. Bowels acted upon. Three motions. Urine spontaneously discharged. Perineum not examined.

17*th October.*—Pulse 100, temp. 101·8°. Urine has to be drawn off, and continued to be so till the 24th. Perineum not examined. She complains of pain in the external genitals.

18*th October.*—Pulse 96, temp. 100·5°. Perineum not examined.

19*th October.*—Pulse 96, temp. 101·4°. Slight attack of phlegmasia dolens in left leg. Perineum not examined.

20*th October.*—Pulse 88, temp. 99°. Perineum examined, and found to be completely divided in its whole length, the fissure extending through the sphincter and for an inch above the verge of the anus. The newly observed wound was clean, but small portions of soft slough were observed about it. It must be remarked that the part had not been examined by me for five days, and was regularly washed by the nurse in attendance. Except this great fissure no appearances of disease were observed.

When she left the hospital the appearances were as before, closely resembling those found after an ordinary laceration of the same extent produced primarily, by over-distension during the birth of the fœtal head.

The further history of the case presents nothing of special interest. The patient was dismissed from the hospital on the 30th October in a convalescent condition, and with advice to undergo an operation for restoration of the perineum after some time had elapsed.

The only occurrence, which I know of, analogous to what took place in this last case, I shall now adduce. I may say, first of all, that there is no analogy between it and the ordinary vesicovaginal or other sloughings, the result of continued pressure; for on the affected parts here there was only momentary pressure. In an ordinary laceration of the perineum of the second degree, it is not rare to observe behind the gaping laceration a line of split skin, such as was seen in this case to extend to the anus. This split skin almost invariably heals up, along its whole extent. In an ordinary laceration of the perineum, with or without split skin beyond the open wound, it is often observed that, as in my case, the vagina is not torn so far backwards as the perineal skin. But in these cases the vagina is generally (sometimes the reverse is the case) found to slough away or retract as far back as the gaping skin wound. In other words, the gaping skin wound does not generally heal up forwards till it comes to the posterior margin of the vaginal wound; but the vaginal retracts or is destroyed backwards till it comes to be in line with the open skin wound.

ARTICLE II.—*On Excision of Isolated Bones of the Tarsus, preserving a Useful Foot.* By GEORGE BUCHANAN, M.A., M.D., Professor of Clinical Surgery in Glasgow University.

(*Read before the Glasgow Medico-Chirurgical Society.*)

SEVERAL cases having recently occurred in my practice in which excision of isolated bones of the tarsus has been followed with a fortunate result as regards the use of the foot, I take the opportunity to bring the subject before my professional brethren, as one deserving of the most careful consideration. There is nothing new or uncommon in the proposal, but I fear that there is a very general tendency on the part of surgeons to the belief that disease of the tarsus is so apt to attack more than one bone, that in most cases it is better at once to amputate at the ankle than to perform a partial operation which may soon be followed by the other.

And I believe that the great success which attends amputation at the ankle is one of the causes of this conclusion. Few operations in surgery of similar importance can compare with that referred to. Most surgeons in hospital practice can quote scores of cases successively without an unfortunate result; and this almost certainty of success is very apt to induce a surgeon to sacrifice the foot rather than submit the patient to the other, with success only problematical and recovery always tedious.

Now, I am perfectly aware that in certain forms of disease, amputation is the preferable proceeding, even though only one bone be affected, and equally convinced that in such circumstances, if the bone be one of the anterior row, Chopart's operation should not be chosen. At present I leave out of consideration disease of the metatarsus and phalanges—I am limiting my remarks to the tarsal bones. Examples are constantly occurring of partial operations being followed by a recurrence of disease demanding amputation. On the other hand, I have seen an amputation performed in a case in which the subsequent examination of the foot disclosed a condition in which, had a partial operation been chosen, recovery would have been certain.

An easy way to get out of the difficulty, of course, is, to do the partial operation first, and, if not successful, to proceed to the major—or to begin the incision so as to remove the diseased bone, and then, if it turns out on closer examination that the adjoining bones are implicated, to go on to the amputation. And in some cases it is necessary to adopt this plan. But, if at all possible, it is much more satisfactory to determine beforehand the extent of the operation you propose to perform.

Now, there are certain conditions of the bones of the tarsus in which it is hopeless to attempt a partial operation, even though disease be limited apparently to one bone. Spontaneous idiopathic caries commencing in the bone substance—most commonly in strumous constitutions—usually makes its appearance in one bone, and it may go through its stages of swelling, softening, suppurating, ulcerating, erosion, discharge; leaving the characteristic sinus with puffy lips, and all the time there may be no evidence of any adjoining bone being similarly affected. If such a bone be excised, the cartilage on its articular extremities may be apparently sound, and the cartilaginous coverings of the articulating bones may give no evidence of disease, but in a few weeks the disease appears in other bones much as it did in the first, apparently in the same spontaneous way. The disease did not spread from the bone first removed, but was latent or even partially developed simultaneously. Many a time have I demonstrated to the students, in the operating theatre, the statement above made. After amputation of the foot for caries, apparently limited to one bone—say the scaphoid—I have sawn the foot longitudinally from the great toe to the os calcis, and similarly along the outer third of the foot, and shown every articular cartilage in the tarsus, of its proper colour and smoothness, and yet the bones, every one, in a state of incipient or partially advanced caries. In such a case, excision of the first affected bone will be of no avail, for the disease is already in the other bones. The only exception to this general statement is the os calcis, which sometimes becomes affected with caries, independent of the rest of the tarsus, and may be removed. Of this more in the sequel.

With regard to caries arising in the tarsus, as the result of a wound or other injury, the conclusion is not to be so sweeping as in the former case, and the opinion must be guided a good deal by the general constitution of the patient, and allied circumstances; but it must be borne in mind that such injuries, though only primarily affecting the part of the foot on which they were inflicted, may be the means of lighting up the disease in the whole tarsus.

But though caries, either spontaneous or as the result of accident, is by far the most common disease of the tarsal bones, yet not unfrequently they are affected with necrosis. The bone is denuded of periosteum, and a probe introduced through a sinus detects a bare rough surface; but it is hard and gritty, not spongy and porous, like a carious bone. It is very often dark in colour, and on being removed and washed has much the appearance of a cinder. The bone often lies in a bed of velvety granulations, which have sprung up to isolate it from the adjoining tissues. Such a case I have known to be mistaken for caries, and have seen a foot amputated, from which, when thrown down, the isolated necrosed bone fell out, having been lying loosely in its bed of granulations. In such a case, excision of the bone, even though it be held pretty firmly in its place by the surrounding bones or tendons passing over it, could be undertaken with a certainty of a good result. This form of disease in bones of cancellated tissue is most commonly the result of accident, which sets up suppurative periostitis leading to necrosis.

Here, then, are two distinct forms of disease—viz., spontaneous caries, and circumscribed necrosis of the tarsal bones—which, when recognised, demand, the first, amputation of the whole foot; the second, excision of an individual bone.

But while the above opinion guides my practice in cases so well defined as those to which I have alluded, there are others in which partial operations will remove the disease and save the foot, especially in reference to disease of the os calcis and in young persons. The discussion of this subject, however, will lead me away from the special object of this paper, which is to give an account of some cases in which removal of isolated bones of the tarsus has been followed by most gratifying results as to the preservation and use of the foot.

1. *Excision of Astragalus.*—Annie M'Gregor, age 27. Admitted 30th August 1875. Three and a half years previous to admission,

patient suffered a severe sprain of the ankle-joint. This was attended to for a few days, but absolute rest was not observed, as she began to walk about shortly after the accident. During the year which followed, pain was almost constantly experienced in the joint, but not of a character so severe as to render her unable to walk. The pain, however, after this time became more intense; so much so, that she could not even place her foot on the ground or exercise the slightest motion, without increasing it. At this period, also, there occurred a swelling of the joint, which was ultimately lanced, thus giving exit to a large quantity of pus, which continued to be discharged for some time after from the openings which resulted.

On admission, the joint and foot were much swollen. Some pain was complained of on movement of the joint. There were two small openings seen, one situated on the anterior, the other on the posterior aspect of the external malleolus, and giving exit to a small quantity of matter.

Treatment.—Hot fomentation, small blisters, and rest, by the limb being confined in a M'Intyre's splint.

1*st October.*—Although the patient has experienced some relief by the perfect rest of the joint, in other respects there was no visible change. The probe introduced into the sinuses detects bare bone rough and hard, indicating that the astragalus was denuded of periosteum, though it had not the spongy feeling produced by a carious bone.

8th Oct.—Excision of the astragalus was performed as follows, after the manner described by my father in the volume of the *Glasgow Medical Journal* for 1854:—

I made a semilunar incision on the outer aspect of the ankle, commencing at the outer edge of the tendo Achillis on the level of the external malleolus, and extending below the malleolus as far forward as the peroneus tertius' tendon. This was dissected up, and the tendons of the peroneus longus and brevis divided. The external malleolus was then sawn through, and the foot twisted inwards till the upper surface of the astragalus looked outwards. No difficulty was experienced in disarticulating the astragalus from its connexion with the os calcis and removing it. The upper surface of the calcis was a little rough, so I removed with a saw a thin slice of that bone. The articular surface of the tibia was covered with granulations, and did not require to be interfered with.

The foot was now replaced and the wound dressed antiseptically. Perfect rest was secured by the limb being placed in a M'Intyre's splint.

The recovery was tedious, but on the 4th February, when the patient was shown at a meeting of the Medico-Chirurgical Society, the cavity left by the removal of the astragalus was quite filled up, and the patient able to exercise flexion and extension of the joint.

2. *Excision of the Astragalus for Necrosis, after complete Dislocation backwards and Twisting of the Bone.*—This case is reported in Mr Hancock's book on the Surgery of the Foot, page 247. The particulars need not be here repeated, but it is sufficient to state that the patient recovered, with a useful foot, being able to walk without the aid of a stick.

3. *Excision of the Cuboid Bone.*—E. Barbour, aged 5, got her foot strained in November 1874 by her mother drawing a tight boot rather quickly from her foot. Shortly after, a swelling appeared on the outer side of the foot, which softened and burst, discharging pus. Since then, a sinus has remained, opening, though occasionally nearly closing. In May 1875, Dr Chalmers asked me to see it, and on introducing a probe into the sinus, the cuboid bone was distinctly felt—bare, rough, but not spongy, while the bones around seemed unaffected.

Excision of the cuboid was very easily performed by making a crucial incision, of which the sinus was the central point. When the flaps were turned out, there was no difficulty in levering the bone out of its place. The recovery was tedious, owing to the formation of small abscesses in the sole; but ultimately the foot was restored to all its functions, and at present there is little trace of the operation. I have no doubt that part of the cavity is filled up with granulations from the periosteum.

4. *Excision of the Os Calcis.*—J. Fulton, aged 8 years. This case occurred several years ago. The patient was placed under my care by Dr Fergus. He received a strain of the ankle, which, in a few days, was swollen and painful. On the third day, there was a large red fluctuating swelling all around the os calcis. I placed the patient under chloroform, and made a long incision along the outer edge of the os calcis; a large quantity of pus escaped, and I found the bone bare and rough. I made a second incision from the first to the external malleolus, and, turning back the soft tissues without trouble, removed the bone from its connexion with the astragalus and cuboid, and extracted it. The recovery as usual was tedious, but the foot was preserved. (The patient, who was present, walked up and down the hall.)

5. *Excision of the Os Calcis.*—J. H., aged 12, was admitted to the Western Infirmary on the 23d July. There was the history of an abscess which had formed on the outer aspect of the heel, had burst, and the opening remained patent.

On admission, there was a sinus in the outer aspect of the heel through which the probe can be introduced, and it detects the bone bare and rough, but not spongy.

Dr Patterson, who had charge of my wards during my holidays, scooped out the bone with a gouge, in the hope that the parts would put on healthy action and granulate. On the 11th October, there was no progress toward recovery, and the bone was found in much the same state as on admission. I therefore proceeded to excise the os calcis by a method which I have followed on previous occasions, and which is described in Mr Hancock's book at page 414. The incisions are made so as to leave the integuments and soft parts suitable for conversion into a proper covering for amputation, if the disease is found more extensive than anticipated; also to avoid the vessels and nerves of the foot. The operation is conducted as follows:—Enter the point of a strong knife at the external malleolus, carry it down perpendicularly to the outer edge, and then two-thirds across the sole, about an inch and a half in front of the heel. At right angles to this make an incision along the outer side of the foot at a point a little in front of the calcaneo-cuboid joint. The two angular flaps are to be dissected off from the bone till the external and inferior surfaces of the calcaneum are nearly exposed. The posterior flap can now be turned back over the projecting heel and the tendo Achillis cut. By dividing the middle fasciculus of the external lateral ligament, the bone can be twisted so as to expose the calcaneo-cuboid articulation, which can be opened with the point of the knife, and then the whole bone can be forcibly twisted inwards, the interosseous ligaments between the calcaneum and astragulus being divided as they are put on the stretch. When the disarticulation is completed, the soft parts, with the vessels and nerves on the inner side, can be separated without any risk of injury. The operation was done antiseptically, and dressed in the same way. By the 17th December, the wound had completely closed, and the gap caused by removal of the bone much filled up—probably the periosteum, which was in great part stripped from the bone along with the soft tissues, was the source of some of the granulations with which the cavity was filled up. The patient was able to walk with ease, flexion and extension of the ankle being almost perfect.

These examples are all of necrosis of the tarsal bones, and my experience of excision with a fortunate result is limited to that form of disease, except in the case of the os calcis. The last is an example of a form of necrosis to which I have applied the term "progressive," and which is described in a letter to Mr Hancock, and quoted in his book, page 395.

For some reason, true spontaneous caries of the calcaneum is not so apt to be accompanied by similar disease in the other tarsal bones as when it appears first in another bone, and when caries is lighted up by injury, it is longer limited to the os calcis than in the case of another bone. Hence gouging out the calcaneum in cases of caries sometimes is sufficient to remove the disease, and in more extensive examples, excision has saved the foot.

I consider, then, that disease of the os calcis is an exception to the general statement that true caries of the tarsal bones demands amputation. In a few instances, I have saved the foot by gouging, and in others by excising a carious os calcis; but I have not been able to get a patient to exhibit as an illustration, and therefore do not intend to allude further to the subject.

The purpose of my paper is served if I have demonstrated by actual examples that a useful foot may be preserved after pretty severe mutilation, if the proper cases can be selected.

ARTICLE III.—*Royal Edinburgh Asylum Papers. Disorders of Speech in Insanity.* By T. S. CLOUSTON, M.D., F.R.C.P.E., Physician-Superintendent, Royal Edinburgh Asylum.

THE muscles which "give expression" to the face, and those which give vocal expression to the thoughts in speech, are obviously far more intimately connected with the higher nerve-centres of thought and emotion in the brain than any other groups of muscles in the body. Ever since the mind functions have been localized in the convolutions, no one has doubted that the motor centres for the co-ordination of the facial muscles must be in most direct and intimate connexion with them. To my own mind, the complete and infinitely subtile power of muscular movements that, in the countenance, represent and respond to every varying shade of intelligence and emotion, was the best proof of the importance of the experimental researches of Ferrier and Hitzig. From the earliest times, when madness was described by poets or physicians, what we should now scientifically call its facial motor symptoms, were graphically portrayed.

The process of deduction that connects the mental state of the man with his expression of face has always been an instinct of humanity. So much is this the case, that, if we want powerful and true descriptions of the outward symptoms of insanity as seen in the face, we must go to the poets and dramatists rather than to the men of science and medicine. Indeed, anything like a scientific account of the effects of mental disorders, as it changes, distorts, and obliterates the normal muscular movements and co-ordinations in the face, is as yet entirely wanting. The whole matter has acquired a new physiological interest since Ferrier's researches were published.

Next to the expression of face, as connecting mind and motion, come the muscular movements and co-ordinations of speech. These are developed *pari passu* in the child with the mental faculties; and ordinarily there is no other leading symptom that so surely indicates a want of structural and developmental power in the convolutions to produce normal and vigorous mental manifestations as an incapacity for speech in a child. The disorders of speech, and their pathological causes, have received much attention of late years. Yet it seems to me, that this attention has been far too much concentrated on the striking phenomena of aphasia. Valuable as have been the results of the study of that disorder, yet they would be even more important if we were able to take a wider view of all the affections in which speech is in any way interfered with, trace them to their pathological causes, whether high up in the brain or low down in the mouth, and connect them with the other diseases they accompany. This cannot be at all fully done until the disorders of speech that are connected with many forms of insanity are studied and more minutely described than they have hitherto been. It is merely following the physiological connexion between speech and mind to study them together. And the more we study this connexion in disease, the more frequently we find that the one function is implicated along with the other. In most cases of dotage, there is just as great a want of capacity to articulate clearly as there is to think clearly. As in the imbecile, where the motor and mental neurine have both been arrested in their development in the convolutions, so in the stage of senile degeneration they fail together.

I propose, in the present paper, to state the general results of my clinical observations as to the kinds of speech affections that occur along with, or, as I should prefer to put it, as part of the symptoms of certain forms of, insanity, rather than go into a detailed study of any of them. I cannot, of course, avoid referring to pathological and physiological explanations of the symptoms in different cases. In fact, if I did what I should like to do, I should endeavour to correlate the facts that have been made out by the recent writers on aphasia and those on brain function, with those that we daily come across in the wards of our asylums for the insane.

In considering such a subject as the disorders of speech that form part of the neurotic symptoms in the various forms of insanity, we may look at it from very different points of view. The aspect that occurs to most of us is the clinical; and the questions we have to solve are usually—What forms of mental disorders have speech troubles as part of their symptoms? What special forms of speech trouble go with special kinds of insanity? What are their significance in our prognosis and treatment? A pure physiologist, again, would look at them in quite another way, and ask—How do those disorders of speech illustrate the motor functions of the brain? How do they help us to connect the motor and mental functions of the brain as to their mode of production, and as to the localities where each originates? A pathologist would further ask—Are they of the nature of convulsion, want of muscular co-ordination, or paralysis? Are they central or local? If central, what part of the brain is involved in the disease? If local, are they muscular or nervous, and which nerves are affected? A combination of all three modes of viewing our cases would, no doubt, be the best; but it is not given to all of us to be able to take such a comprehensive grasp of each case. We shall, no doubt, do most service to our patients, and in the long-run, perhaps, to pathology and physiology too, if we mainly bend our mental energies to the clinical views of our cases, observe the clinical facts, and, when opportunity offers, observe carefully the pathological anatomy. But this need not quite prevent us trying to look at our cases from the other aspects.

Disorders of speech—and, of course, in that term I include articulation, as the greater includes the less—are met with, either in all or some of the cases, in the following forms of insanity, viz., general paralysis, epileptic insanity, choreic and rheumatic insanity, syphilitic insanity, congenital insanity, the insanity that accompanies tumours of the brain, the insanity accompanying hæmorrhages and local softenings of the brain (organic dementia)—and in this may be included that accompanying aphasia—the insanity accompanying alcoholismus chronic and acute, senile insanity, certain forms of insanity with acute delirious mania, melancholia atonita, and acute dementia, pellagrous insanity, and, finally, in a form of insanity that I have not seen described, which accompanies a rare kind of trophic anasarca and thickening of the skin, resembling elephantiasis, only that it is general over the body, and is accompanied by a peculiar kind of partial paralysis of the arms and legs.

A very complete account of the kinds of speech affections in each of those disorders could not be given in the limits of this paper, and the present state of our knowledge of normal brain function does not enable us to connect in an absolutely definite way the clinical symptoms with a definite pathology in many of them.

The first form of insanity, or of brain disease, if the term is preferred, viz., general paralysis, is, on the whole, the most interesting of all as connected with speech affections, both because of their constancy and specialty. The disorders of speech are more a part of this disease than the mental symptoms. If the speech, in a typical case of general paralysis, be carefully studied from the beginning to the end of the disease, we find that there are three phenomena which, in different degrees, are always to be seen—1. tremor in each separate muscle, 2. want of co-ordination between the different muscles, and, 3. advancing paresis of the muscles of the lips and tongue. The first motor symptom of the disease is, in a vast majority of cases, slight tremors or convulsions in the muscles of the lips, or running down the longitudinal muscular fibrillæ of the tongue, with an occasional slight want of co-ordination once in a long sentence when those muscles are put in action during speech. It is rare that, in the first stages of the disease, those tremors take the form of a general spasm of all the muscles so as to produce stuttering in the speech. I have only met with one or two cases of the disease which had a typical stutter, and it is doubtful whether they did not stutter before, this being aggravated by the general paralysis. In some cases, the tremblings are to be seen in the fibrillæ of the labial muscles, but in most cases they are to be seen equally, if not more distinctly, in the tongue. Mere tremors confined to the tongue, however, in no way indicate the disease. It is a very striking and remarkable fact, to which special attention has scarcely been directed, that the area of those tremors, and of the first want of co-ordination, is entirely confined to the motor nerves that take part in the function of articulation; and the muscles that supply the other cranial motor nerves—viz., those to the eye, to the muscles of expression in the upper part of the face, to those of deglutition, of voice, or the pneumogastric—are not ordinarily affected till later on, either with fibrillar tremors or want of co-ordination or paresis. In different cases the labial muscles and the lingual are affected in quite different degrees, so that we cannot apply any test words or sentences that will bring out the weak points in the speech of all general paralytics. In some cases they slur the labials, and can articulate the linguals distinctly. In most cases the words which a commencing general paralytic is most apt to trip on are those with *f's*, *th's*, many *r's*, and *t's*. The pure labials, such as *b*, are nearly always articulated, the *m's* and *n's* and the gutturals and *h's* always so. The facility with which the *h's* and *sh's* are said, distinguishes the diseased articulation of this disease from the ordinary stutter. The general paralytic always makes most blunders towards the end of a long sentence, or when hurried or excited. When deliberately trying to speak slowly and distinctly, especially if the sentences are short, and the same series of consonants do not recur often, he speaks best.

As the disease proceeds, the tremors, which at first were confined to distinct and separate fibrillæ of each muscle, invade all its fibres when they are put in action. Want of co-ordination between the combined actions of different muscles is observed, and the first signs of actual want of power or paresis appear. The first part of a word can often be said, but not the last, the effort to articulate which is merely represented by a general motion of the lips, and the vowel sound of the word only is heard. It is at this stage, and only at this stage, that the palate muscles, the guttural sounds, and the laryngeal muscles become affected, while at the same time there is often nystagmus and quiverings of the upper facial muscles.

After congestive or epileptiform attacks, as is well known, the defects of articulation are much more marked, and generally advance much more rapidly at that time. And it is also well enough known but not sufficiently dwelt on, that the articulation may recover itself for a time, so that a man, who could not speak a sentence without betraying his disease, may talk for an hour and not make a slip. This is a most important point to be kept in view in considering the pathology of the affections I have been describing.

In the second stage of the disease the want of co-ordination is very choreic in its character, but with the convulsive tendency in addition. Tell a general paralytic in the first stage to put out his tongue, and he at once does so; but you see quiverings running down groups of the fibrillæ of its muscles. Tell the same man in the second stage to do so, and he puts it slowly out, the whole organ being pushed about in an unsteady way, through its muscles not acting harmoniously towards the desired end. Tell him in the end of the third stage to do so, and the only response is his moving the organ about a little without being able to protrude it beyond the mouth. Any one accustomed to see much of the disease can often diagnose it, and the stage it has reached, from seeing the tongue alone.

The third stage is distinctly paretic, and then fully paralytic; but long before this stage is reached there is want of power and also want of will in the patient, if he is free from excitement, to speak much at all. He still has some power of speech, but he does not exert it much; and in some cases this is so marked that it requires a great stimulus indeed to make him put forth what power he has.

There are a great many cases of the disease where distinctly aphasic symptoms are present. Most frequently those are present for a short time and then disappear. In one sense, of course, all the cases of the disease in the last stage are aphasic; but what I mean is, that the patient loses the power of speech, not from advanced local paralysis of the lips and tongue, but from a special inability to articulate or say certain words. For instance, I lately had a patient who, in the very first stage of the disease, one day lost the power of speech. He could walk about, eat, read, and write, but he could not speak at all in answer to questions; and then, next day, as he was regaining the power of speech, called things by quite wrong names. Could not say "dog" or "cat," though he knew those were the names of the animals before him, and assented when they were called so. No other symptoms were present except a slight rise in the temperature of his body, and a very slight amount of right hemiplegia. As the aphasic symptoms passed off, he went into an attack of excitement, with much tremor of the lips and tongue, and delusions of grandeur; but those symptoms again passed off, and he remained for a time so well that his relatives thought he had recovered, and removed him from the Asylum. After congestive attacks, aphasia is common enough as one of a number of its symptoms, *e.g.*, general confusion of mind, want of attention, restlessness, etc.

Speaking generally, the affections of speech run parallel to the mental disturbance in general paralysis; especially the exacerbations of the disease, the congestive and epileptiform attacks, affect both the speech and mind. The first stage of the disease, when there is much fibrillar muscular tremor, and an occasional want of muscular co-ordination in speech, is the convulsive stage, corresponding to the semi-rational restless impulsive and grandiose stage of the disease. The next stage, of considerable want of co-ordination, or the choreic stage, corresponds to the facile, excited, and quite irrational stage, when there is little self-control left; and the third or anergic stage corresponds to the mentally fatuous condition that characterizes the end of the disease.

Now, what is the pathological seat of the speech affections in general paralysis? Nearly every pathological fact, and certainly every fact of recent physiological experiment on the brain, would lead us to the conclusion that its seat is in the brain convolutions at first, and not in the lower motor ganglia of the brain. The intimate association of the speech affection, from the first, with the mental affection, the peculiar character of it, the marked distinction of the speech of general paralytics from that of patients who suffer from local lesions lower down than the convolutions, its close analogy, as we shall see, to the affections of speech resulting from alcoholic poisoning, or from general pressure and irritation of the convolutions by tumours, all go to prove this. But it may be asked, Is the whole series of speech symptoms convolutional? I think we have every reason to suppose that the want of co-ordination between the different muscles of the lips and tongue is the result of the progress of the pathological brain degeneration down into the corpora striata and the nuclei of the motor tracts of the seventh and ninth cranial nerves. M. Voisin, in a paper in the *British Medical Journal* for 19th June, has attributed the complete paralysis at the end of the disease, to fatty alteration of the muscular fibres of the tongue. The muscular fibres of the tongue may undergo fatty degeneration in the end of this disease; but I do not think they do so more than the other voluntary muscles, and M. Voisin seems to me so entirely mistaken in his pathological physiology when he attributes so much of the speech troubles to the microscopic changes he found in the medulla, and so little to the changes that others have demonstrated in the convolutions, that one hesitates as to accepting any of his general conclusions on the subject. It seems to me that we can now regard it as a settled fact in brain pathology, that where we have any sort of tremor or convulsion of the muscles supplied by the cranial nerves, such as we see in the first stage of general paralysis, its source is in a lesion of the convolutions. And if the area of this tremor is limited to the muscles that subserve a special function, such as speech, our conclusion may be regarded as much confirmed; for where are such areas mapped out functionally but in the convolutions? And if such an affection is aggravated at one time, and then almost recovered from, our conclusion is also strengthened. Any sort of motor spasm or paralysis, or want of co-ordination originating lower down, seldom comes and goes in that way.

It is to be kept in mind that these tremors only occur in ordinary cases when voluntary motion is being performed or going to be performed. The nerve force is being sent down from the central battery, but seems to come in an unsteady gusty way, each nerve fibre transmitting a different strength of current from its neighbour. Though I cannot agree with the late Dr Thompson Dickson that the natural tendency of the central motor nerve batteries in the brain is to cause tonic contraction of the muscles, which is only prevented by some other inhibitory nerve apparatus, yet the true physiology of the series of quick successive muscular contractions that take place in tremor does remain obscure. The tendency of the tremors to be aggravated the longer a voluntary effort is continued in general paralysis, no doubt confirms the opinion that the essential condition of the disease is one of weakness and tendency to paralysis from the very beginning, even if we had not known that such tremors were the natural halfway-house to paralysis.

The purely aphasic symptoms are engrafted on the general and ordinary ones where they exist, and may be regarded as indicating that the convolutions in the posterior parts of the left anterior lobe are specially congested, or in some way having their function interfered with. My own opinion, formed from clinical observations, is, that the speech area in the convolutions of the brain is a large one, situated in the anterior lobes, speech being a very complicated series of fine muscular movements, but that there is probably one special area or convolution whose function it is to act as a medium between the cells of ideation and those of speech-motion, and give the general impulse to the area through which the co-ordinate movements of speech take place. In general paralysis we have this whole area affected, and if this special convolution or series of brain cells are interfered with, then we have the symptoms of what is called aphasia. A case which I shall relate afterwards, under epileptic affections of speech, bears on this point. If we imagine this convolution had intimate relations to the area in the convolutions that regulate the co-ordinate movements of the fingers, we may form some conception of what would produce agraphia.

The disorders of speech that are sometimes associated with epileptic insanity have a distinct pathological relation either to the disturbed action of the brain following fits, or to the diseased condition of brain that causes the fits. The disorders resulting from the first cause are by far the most common, and are of course temporary and recurrent in their character; their intensity usually bears a direct relation to the intensity or number of the previous fits. They are of two kinds, which in some cases may be combined. In one set of cases, after the patients have had a very bad single fit or a series of them, it is found that for the next twenty-four hours the tongue is tremulous, and there are occasional twitchings of the face when they begin to speak; they articulate very slowly indeed, and somewhat indistinctly, as if from want of a powerful enough nerve current being sent down from the brain to the muscles of articulation. After a few words have been articulated there is a hesitation, and then the patient stops in the middle of the sentence and cannot go on, this appearing to result partly from his forgetting what he was going to say, and partly from motor exhaustion in the muscles of speech. There is, in fact, a combination of convulsive paretic and aphasic motor symptoms, with enfeebled ideation and loss of memory, all these being, in my opinion, clearly traceable to exhausted energy of the convolutions. This form of speech disorder is distinguishable from the general paralytic speech by the absence of fibrillar tremblings in the muscles of the tongue and lips, by the absence of the want of co-ordination in articulating certain words, a nerve slowness taking its place, and by the presence of the aphasic symptoms, which are certainly rare in the first stage of general paralysis.

The second form of speech disorder, which I have observed in three instances in epileptics, was a very interesting one. It only occurred after a series of bad fits, and consisted in a constant tendency of the act of articulation suddenly to pass into a violent hiccough, and of course to be brought to an abrupt termination thereby. In one of those patients, a young man who had suffered from epilepsy from childhood, was very weak-minded, spoke quite well at ordinary times, and got very talkative and excited after he had an unusual number of severe fits in succession, but yet he could never get further than the first few words of the sentence he was going to say, being suddenly brought up by a most severe hiccough that shook his whole head and chest. He would then begin the sentence again with the same result, and this most ludicrous performance would go on for five minutes, *e.g.*, "Oh, doctor! I want"—hiccough; "oh, doctor! I want"—hiccough, etc. By next day, this tendency was gone. The other two cases were exactly like this in their symptoms. Now, it seems to me that in such a case, the cause of the convulsive hiccough was probably a disturbance in the action of the reflex centres of the pneumogastric and recurrent laryngeal nerves down in the pons, and not in the convolutions at all. The well-known congestion that takes place in the pons during a fit, may account for it. I have never seen this symptom except in very long-continued and bad cases of epilepsy—such cases, in fact, where we always find after death congestion and dilatation of the vessels in the medulla and pons.

The speech affections that are directly dependent on the pathological lesions of the brain which also cause the epilepsy are always of the paralytic or aphasic kind, or a combination of both. They vary much of course according to the seat and extent of the local disease. One of the most instructive examples of this I met with lately. A woman had taken fits for thirty years, had been four years in the Asylum, and was subject to attacks of restlessness and mental excitement with incoherence after the fits. At all times her lips projected and hung loose, especially the lower one, and there was a partial paralysis of them both, which was most distinctly seen in the way she slurred all the labial consonants; she could by an effort say certain labials, such as *m*, but those where the lower lip is specially called into play, such as *f*, she could not say at all. Her tongue seemed to be quite normal in its mobility. After having a bad fit or a series of them, she became quite aphasic. She could understand what was said to her, and would try to answer, but could not do it. Her lips would move, and sounds would be uttered from the throat, but no articulation of any kind by lips or tongue could be performed. In the course of a day she usually regained gradually the power of making herself understood, but it was a few days before she regained her power of speech. During the process of recovery the lip movements were always the worst. She died a few weeks ago, and I found in the centre of the white substance of both hemispheres softened spots opposite the anterior ends of the corpora striata, that on the right side being the size of a bean, and that on the left the size of a pigeon's egg. The softening on the left side extended outwards and involved the convolutions of the island of Reil, and a portion of the third anterior convolution (Broca's). Round each of those softenings the brain substance was pale, and of less than normal consistence.

In this case the softening distinctly encroached on Broca's convolution, but did not affect the extra-ventricular nucleus of the corpus striatum. It is evident that the motor innervation of the lips was always affected, not that of the tongue till after the fits, and we may conclude that the softenings were the cause of the former. Was it the softening outside the corpus striatum, or that of the cortical matter? I am strongly inclined to put down the latter as being the cause. The epilepsy, I also attribute to this convolutional degeneration. Why was there complete aphasia after the fits, while there was merely partial labial paralysis at ordinary times? Probably because the "discharging lesions," or the fits, originating in and around the diseased convolutions, would specially involve the neighbouring parts in the exhaustion and damage to function that a fit ordinarily causes. In that way not only the convolutions specially regulating the labial movements of speech would be affected, but also those near them with a still higher function, viz., the setting into co-ordinated action the whole speech area in the brain, in accordance with the "movements" of the purely mental brain cells. At all events, the case is one singularly instructive, and, I think, almost unique, as showing the complete distinction between pure aphasia and mere labial paralysis, and yet demonstrating how close the connexion is between the two, both functionally and pathologically. Had the softening involved the whole of the third frontal convolution of the left side, one can scarcely doubt that both the aphasia and the labial paralysis would have been present at all times, and not merely after a large number of fits.

In choreic and rheumatic insanity the difficulty of speech is of a totally different kind physiologically from that seen in general paralysis and epileptic insanity. There is no spasm, tremor, or convulsive tendency in the true sense, but a simple want of co-ordination between the different muscles of the tongue and lips, which is not at all specially confined in any case that I have seen to the speech area, but extends to the muscles of the features, and head and neck. The speech is as indistinct as if the mouth were full, and it is also jerky. It clearly results from disturbance in the working of the regulating groups of motor nerve cells much lower down than the convolutions. There is no special difficulty in pronouncing labial more than lingual consonants, or linguals more than gutturals.

The disorders of speech that accompany syphilitic insanity are determined in their character by the position and extent of the local lesion. If the gummatous deposit or softening interferes with the structure of the convolutions, the affections of speech closely simulate those of general paralysis or epilepsy—indeed, in those cases, there are always epileptiform convulsions. If the lesion is near the third anterior convolution of the left side, there are almost always aphasic symptoms, but they have no special feature. As Dr Hughlings Jackson has so well pointed out,[1] the neuroses caused by syphilitic deposits in the brain have no specific character. They attack not the nervous elements proper, but the vascular or fibrous, and injure the structure and function of the nerve cells and fibres by pressure, deprivation of blood, or irritation. My own experience has been that an extreme slowness of speech always follows the epileptiform attacks caused by syphilitic deposits.

The affections of speech that accompany the congenital forms of insanity are caused either by want of development in the organs of speech, which is rare, or by want of development in the co-ordinating motor centres in the brain. There is a curious mode of speaking seen in some imbeciles—not quite a stutter, but something like it, with a great tendency to repeat the last words of every sentence. This was clearly the speech on which Sothern founded that of Lord Dundreary, and he was wonderfully true to nature. But the disorders and defects of speech in idiocy and imbecility I do not propose to enter on at all here.

The speech, as it is affected by some tumours of the brain which are situated high up and press on the convolutions (and these are the tumours that cause insanity), resembles that of general paralysis in the most striking way at first, and afterwards, when they press more on the corpus striatum, that of ordinary hemiplegia and white softenings. This likeness of the articulation in the case of tumours in this position to general paralysis, I think, confirms strongly the convolutional origin of the latter in its early convulsive stages. I have always observed a thickness of speech, too, resembling that of intoxication, in persons with tumour of the brain affecting the speech.

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[1] Journal of Mental Science, July 1875.

The impaired articulation of the man who has had an ordinary attack of hemiplegia, from softening or apoplexy into the corpus striatum or its neighbourhood, is very well known. It forms one of the symptoms of the disease, like the mental symptoms that occur in some of the cases. The contrast between this kind of articulation and that of general paralysis is most marked, and very well illustrates the difference between what, pathologically, might be called ganglionic and convolutional affections of speech. In the one case there is of course the want of power in the muscles of articulation on one side, and this gives a thickness, slowness, and want of precision, to the articulation; especially the labial consonants are not sharply pronounced. But there is no trembling, fibrillar or otherwise. The tongue is unsteady, to be sure, but not from tremor. The palate gets affected too, so that the gutturals and aspirates are not easily and distinctly said. The patient always gives the impression of its being hard work for him to speak, which is not at all the case in a commencing general paralytic. This kind of speech is essentially a "thickness of speech."

I shall say nothing about the different kinds of aphasia, that being far too wide a subject for such a paper as this.

The disorders of speech in alcoholism, chronic and acute, are most like those seen at the beginning of general paralysis. Looking merely at the speech symptoms, it is almost, if not quite, impossible to diagnose the two diseases from the symptoms alone, without taking the history and course into account. In the alcoholic cases, of course, the tremulousness is not confined to the speech area, but extends to all the muscles of the body. There is in the two diseases a combination of want of co-ordination among different muscles, and a tremulousness in each. No doubt, both the motor apparatus in the convolutions, and that in the lower ganglia and centres, is affected, but the most marked of the speech symptoms are undoubtedly convolutional. The lower centres seem most affected in acute alcoholism, and the convolutions in the chronic form of the disease.

The disorders of speech in senile insanity and extreme dotage are very interesting physiologically. No one can listen to a man in his extreme dotage talking, without perceiving that the motor apparatus of articulation is as much affected as his mental apparatus. When looked at carefully, we see that in the first place there is a certain amount of slowness, indistinctness, and hesitancy of speech, indicating a want of power — a paresis, in fact. Then, in some cases, we see that, like the general paralytic patient, there is a certain amount of convulsive tendency seen in the tremor of the labial and lingual muscles. The first words of a sentence, or the first syllables of a long word, are far more distinctly enunciated than the last, showing an easily-exhausted stock of nervous force, as well as a paresis. Then there is a want of co-ordinating power. The words having many *th* sounds are not properly articulated, *e.g.*, the patients cannot say "The astonishing thing is that those thieves should think this." The power of rolling the *r's* is deficient too. There is also a distinct tendency to reversion in the resemblance of such speech to that of the child learning to speak. Finally, there are well-marked aphasic symptoms in some rare cases. I have a woman now in the Royal Edinburgh Asylum, when she wants to say, "Now, you take that," says, "Now, you ter ter ter." In the cases where a more acute psychosis takes the place of the ordinary dotage, all these motor symptoms of speech are sometimes also much more marked. Sometimes one set of them are more prominent than the others. Pathologically, no doubt, all this means a universally impaired motor structure, both as to cells and fibres, as to convolutions and lower ganglia—an impaired nutrition, a diminished stock of nerve forces produced, and an irregular transmission of that force to the muscles of articulation. The close connexion and entire parallelism between the mental functions and the speech function is never better seen than in some of those cases of extreme dotage. The loss of memory of events is only equalled by that of words; the want of self-control is only paralleled by the garrulousness and the feebleness of purpose, by the tremblings, inability to articulate distinctly, and want of co-ordination in the muscles of the lips and tongue.

The interference with the function of speech in acute delirious mania is often most apparent. The ill-pronounced words, the half-finished words and sentences, and the hurried articulation, are the most obvious symptoms. In some cases I have seen the most intense excitement without any articulate words being spoken at all, and the patients could not be got to speak. The surprising thing is, not that the speech is affected in these cases, but that it is not more affected, when we consider how profoundly the mental apparatus in the convolutions must be disordered, and how closely connected this must be with the motor. The abnormalities of speech in those cases are probably attributable pathologically to want of proper and sustained voluntary and inhibitory power being brought to bear on the motor centres of speech, which are started and stopped hurriedly, and not supervised when in action by the mental and inhibitory centres.

In melancholia atonita, where the patients never speak at all, there seems to be a morbid action going on in one set of brain cells of such intensity that no stimulus to the motor apparatus of speech ever reaches them from the brain, while in acute dementia the speech area in the convolutions probably shares the deadness and inactivity of the rest of the brain. The mutism of pellagrous insanity seems to be of this character too.

ARTICLE IV.—*On Inflammation.* By G. THIN, M.D., London.

(*Concluded from page* 798.)

CHAPTER V.

Two papers by Mr Lister, in the *Philosophical Transactions* for 1858, "On the Cutaneous Pigmentary System of the Frog," and "On the Early Stages of Inflammation," introduced to the notice of pathologists a new field for the study of inflammatory change. Nothing of much moment has been added to Mr Lister's observations, and although the line of research indicated by that author remains one of the most promising, it has been comparatively neglected. This may at first sight seem strange, as the examination of the pigment in the web of the frog's foot is simple enough, but I believe it is to be accounted for by the fact that there underlies this subject one of the most difficult and vital points in histology.

When the web of a frog's foot is examined under the microscope, it is found to contain numerous masses of black pigment. The pigment may be concentrated in isolated circular dots, or form an unbroken network. In the former condition, the skin when looked at by the naked eye appears pale, in the latter dark. Between the concentrated and diffused conditions all possible intermediate stages can be observed. In, or on, the edge of the central mass of pigment a large oval nucleus is frequently to be seen. Hence these bodies have been believed to be cellular, and have been known as pigment or chromatophorous cells. When they passed from the diffused or intermediate stellate form to that of round isolated bodies, this was (and is I suppose still by most authorities) regarded as being produced by a retraction of cell processes, and when they again became diffused, the processes were believed to project until they met and ran together. Here, it was natural to suppose, is an excellent opportunity of studying the changes that take place in cells under different influences. But Mr Lister came to another conclusion regarding these appearances. He found (*loc. cit.*, p. 680) that "the Germans had taken an entirely erroneous view of the phenomenon, attributing the round form of the masses of pigment to contraction of the branching offsets of stellate cells, whereas it turned out that the chromatophorous cells do not alter in form, but that the colourless fluid and dark molecules which constitute their contents are capable of remarkable variations in relative distribution, the molecules being sometimes all congregated in the central parts of the cells, the offsets containing merely invisible fluid, while at other times the colouring particles are diffused throughout their complicated and delicate branches; and between these extremes any intermediate condition may be assumed. It further appeared that concentration of pigment takes place in obedience to nervous influence, while diffusion, though also an active vital process, tends to occur when the pigment cells are liberated from the action of the nerves." Mr Lister found that the action of irritating substances "paralyzed" the pigment "cells," and that so long as the effect of their application remained, although the pigment in the cells in the neighbourhood of the affected part was capable of changing its condition, that which was within the area of the substance applied remained fixed in the original position. It is impossible for me, in the short space at my disposal, to attempt to give even an outline of Mr Lister's experiments. The papers referred to should be read by all who are interested in the study of inflammation.

When I first began to observe the changes in the pigment bodies, I adopted Mr Lister's views regarding their nature, and in the abstract of a paper on inflammation, published in the *Proceedings of the Royal Society*, No. 160, 1875, I compared certain appearances observed in the stellate cells of the cornea to a "collection of the protoplasmic particles in the centre of the cell, similar to that which takes place in concentration of pigment." In the second chapter of the papers published in this Journal, whilst describing the spherical condition observed in the stellate cells of the cornea, I did not give expression to the hypothesis of the concentration of protoplasmic particles, which may or may not be true, and in any case is not proven, and I purposely delayed all reference to the pigment cells, hoping that before these papers were brought to a close an investigation into the nature of these bodies, with which I was from time to time engaged, would be so far advanced that I should be able to express a definite opinion as to their structure. The investigation is still incomplete, but as this is the last chapter I shall write on the subject at present, and as the question is intimately connected with that of inflammation, I cannot pass it over in silence, more especially as my further researches connected with it may be indefinitely delayed.

I have come to the conclusion that these pigment bodies are neither cellular, as usually maintained, nor masses of pigment granules suspended in the fluid contents of cells, as is believed by Mr Lister. Without burdening the reader with a mass of technical details, I may mention that the methods on the results of which I base this view consist chiefly in submitting small portions of the web to a process of digestion, by placing them in the stomach of a newly killed frog, and in treating portions of the web and other parts of the skin by saturated solution of caustic potash, according to the method minutely described by me in the *Quarterly Journal of Microscopical Science* for January 1876. By these methods I succeed in isolating portions of the pigment in small patches and in a complete network. When isolated from the tissue they never contain a nucleus. On the other hand, in partially digested skin the large nucleus is left behind, and is seen to have a fixed and firm relation to the tissue, whilst the pigment has none, either to the nucleus or the tissue. To complete the investigation it is necessary to discover the nature of the cell whose large nucleus is visible amongst the pigment.

I believe that the pigment is contained in the interstices of the tissues, but that at the same time it is in certain parts capable of penetrating the substance of cells, as for example in the choroid. This view, so far from diminishing the interest of Mr Lister's experiments, increases it; for in observing the movements of pigment, we observe some of the changes that take place in the contents of the lymph spaces, a series of changes of which we as yet know almost nothing. In diffusion of pigment we have, as I believe, a natural injection of those intermembranous interstices which Recklinghausen has described as *Saft-kanälchen.* The mechanism by which the pigment accumulates in the dilated central points of these interstices is unknown.

For the sake of completeness I must here further interpolate by adding a few remarks on "giant cells." Any granular ill-defined mass containing in its substance a number of nuclei or nuclear vacuoles, is termed a "giant cell." Such masses are found constantly in the marrow of young bones, in certain tumours of the lower jaw, and in tubercular deposits. They are further found in granulations of wounds however produced (Jacobson), in chronic ulcers (Brodowsky), in typhoid ulcers (Klein), and according to Jacobson they have been described as existing in tumours, glandular swellings, elephantiasis, lupus, syphilitic gummata, abscess of the mamma, etc. To regard these structures as having a specific relation to any particular disease, as has been done for tuberculosis, is unjustifiable. Axel Key and Wallis observed in the inflamed cornea large cells with twenty nuclei which correspond to the definition usually given of a "giant cell." I have never seen such a cell in the inflamed cornea, but I have on several occasions observed in serum preparations masses of conglomerated colourless blood-corpuscles, in which the outlines of each individual corpuscle were so faint that at first I was inclined to consider them as one large mass. More careful examination showed that they were without doubt colourless corpuscles. In osmic-acid preparations this difficulty never occurs.

I have described, in a paper on the Structure of Cartilage, *Quarterly Journal of Microscopical Science,* January 1876, a preparation in which a transverse section of healthy articular cartilage, after being sealed in blood serum for seven months, was resolved into an immense number of the structures designated giant cells. A most interesting series of experiments by Ziegler[1] throw much light on this question. This experimenter fastened two small squares of glass together so that a very narrow space was left between them, and inserted them in the subcutaneous tissue of a dog. The wound closed over the squares, which were dissected out after various intervals of time, treated by osmic acid, and examined under the microscope. The space between the squares was partly occupied by colourless blood-corpuscles in all stages towards fully developed "epithelioid" cells, and by giant cells. So much for the fact observed. Ziegler's theory is that the giant cells are formed from the colourless cells by the development of one cell, which consumes its neighbours, and grows at their expense. The nuclei of the giant cell are not the nuclei of the colourless corpuscles, but nuclei formed by division in the growing "giant cells." This process of consumption and growth is purely hypothetical. Not a single fact is brought forward to support it, and the drawings afford proof that no evidence has been attainable. What Ziegler has shown is, that between his glass plates colourless blood-cells develop into epithelial (or epithelioid) cells, and that later the shapeless masses known as giant cells are also seen. I believe that in Ziegler's experiments, after the epithelium had been formed, it lost its vitality and degenerated into an ill-defined cell-conglomeration. I cannot dwell on the subject, but will take the liberty of stating my belief dogmatically, that "giant cells" are either the product of disintegration of layers of flat cells in tissues undergoing absorption physiologically or pathologically, or are produced by an abortive effort on the part of colourless blood-cells to form such a layer. In disintegrating processes their form will depend to a certain extent on the tissue in which disorganization is taking place, and a "giant cell" may, amongst other things, be simply a dead and disintegrating capillary vessel. In all inflammatory processes the occurrence of "giant cells" is possible, and is in no sense specific.

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[1] Experimentelle Untersuchungen, etc., von Dr Ernst Ziegler. Würzburg: 1875.

When the destructive effects of the inflammatory processes have ceased, the elements which have lost their vitality undergo various changes, and are either discharged from a free surface or are absorbed, and a new tissue is formed which is destined to restore the lost integrity of the part. Every step of this process is as much a matter of controversy as are the primary inflammatory changes discussed in the earlier chapters of these papers. In this instance, however, considerations of space prevent my entering minutely into the histological details, and I shall, instead of criticizing the various doctrines which are taught by pathologists, give a summary of my own views, and the nature of the investigations on which they are founded.

The plasma which escapes from the bloodvessels of an inflamed part coagulates, as is very strikingly shown in pericarditis or pleuritis, when the surfaces of the membranes are covered with the soft yellowish substance, which at a later period is capable of becoming organized and forming false membranes. This substance is coagulated blood plasma, and contains a greater or less number of colourless blood-corpuscles. From these two elements—the formed and unformed, or cellular and non-cellular—the newly formed tissue takes its origin.

The blood plasma contains albumen and fibrine-forming substances, whilst the fibrillary tissue that forms the groundwork of the new growth is largely gelatinous. The mechanism of the formation of this gelatinous tissue is not understood. What can be observed is, that in a transparent homogeneous substance, rich in cells, fibrillation takes place, and with this fibrillation the tissue begins to assume its characteristic and permanent appearance. If accurate knowledge has not been obtained regarding this process, theory has as usual done its best to make the want unfelt. The theories regarding the question are not many, and have their source in the opinions which have long held undisputed sway regarding certain imaginary properties of cells. Some observers describe the fibrillæ of the gelatinous tissue as being given off (I may say shed) by spindle cells. These cells, according to this view, manufacture the tissue out of their own substance, which is renewed by a process of nutrition as fast as it gives off the new tissue. Others attribute the fibrillation to an influence exerted by the cells on the amorphous material around them. Others again believe that the tissue is formed by lymph cells which become transformed into the new substance. None of these theories have passed from the region of hypothesis into that of fact. They are unproved.

I may illustrate the insecurity of the foundations on which prevailing doctrines regarding the growth of tissues rest, by referring to Max Schultze's theory of the formative activity of the cell, and the kind of evidence which has been considered satisfactory in histological questions. Schultze, observing that in embryonic muscle cells are prominent objects, and that in fully-formed muscle, nuclei are to be found in the substance of the muscular fibre, and around the nuclei a scant amount of granular substance, came to the conclusion that the large volume of muscle substance is a product of the cell, the nucleus and a small amount of the cellular protoplasm persisting. The amount of muscle substance formed being out of proportion to the number of nuclei observed, he framed the theory of the formative activity of the cell. Mr Fitzjames Stephen, in his book on Liberty, Equality, and Fraternity, accounts for the complacency with which erroneous doctrines are accepted in ethical science, by a passion in human nature to theorize, and to cling with faithful devotion to any theory which gratifies the love of order inherent in the mind. Some such love of order has prompted the readiness with which Schultze's theory has been embraced by histologists. Dr Beale had previously announced a theory which was perfectly identical, but unfortunately condescended to give a reason. Max Schultze's arguments were simply statements of belief, and criticism was disarmed. Dr Beale disturbed the composure of unreasoning faith by his carmine. Schultze's cell is Beale's "germinal matter," the muscle substance produced by the formative activity of the cell is the "formed matter." What is stained by carmine, forms; what does not stain, is formed. Even its simplicity could not secure acceptance of such a reason. Schultze, as it seems to me, has been awarded too exclusively whatever merit may belong to the theory, which, to say the least of it, is as much Beale's as Schultze's. Neither of these able histologists adduced a single observation in support of it, and it might just as well have been imagined by a poet as by a histologist. In a paper published in this Journal, to which I have several times had occasion to refer (September 1874), I tried to show that the nuclei of muscle are the nuclei of cells that can be observed in and isolated from the muscle substance. For more complete evidence of the same fact, I refer to a paper on the histology of muscle, published in the current number of the *Quarterly Journal for Microscopical Science.* All that Schultze was warranted in affirming was, that there are nuclei in a muscular fibre. I am now warranted in stating that there are nucleated cells in a muscular fibre, and that many of these are applied to the surfaces of its constituent bundles. Of the relation that these cells have to the formation of the muscle substance, if any, we have no knowledge. New methods have shown what the nuclei of muscle really are, and Schultze's theory of the formative activity of the cell must collapse, or find a different basis of support.

Gelatinous fibrillated tissue forms, under favourable circumstances, in the homogeneous plasma that is poured out from the bloodvessels in inflammation. Ranvier has shown that when this tissue forms in the embryo it is distinct from the cells which are mingled with it; and, in a series of experiments I have lately made in cicatrices at various stages, I have always been able to distinguish the cells from the fibrillated tissue. The cells undergo gradual and uniform changes whilst this tissue develops, but it by no means follows that the one set of changes is absolutely dependent on the other. The development of this tissue from the albuminous plasma, which is its matrix, is a question which invites further research. Suggestive remarks on the subject will be found in Paget's "Lectures on Surgical Pathology," and in Cornil and Ranvier's "Manuel d'Histologie Pathologique."

The colourless blood-corpuscles (lymph-corpuscles) that are mingled with the plasma undergo several definite changes, and by means of osmic-acid solution these can be followed with a facility that is impossible by other methods known to us. These cells are the same as those described by Paget as granulation cells; and as I have been able to verify all the stages of transformation described by that author, I will quote his words:—

"The nucleus becomes more distinct; then oval (even before the cell does), and at the same time clearer, brighter, like a vesicle tensely filled with pellucid substance. One or two nucleoli now appear distinctly in it, and soon it attenuates itself; but this it does later, or in a less degree, than the cell; for a common appearance is that of elongated cells bellied out at the middle by the nucleus.

"While these changes are ensuing in the nucleus, each cell also is developing its structure; first becoming minutely, yet more distinctly, granular and dotted. . . . . It elongates at one or both ends, and thus are produced a variety of lanceolate, caudate, or spindle-shaped cells, which gradually elongate and attenuate themselves."[1]

The processes of the spindle cells which are thus formed join each other, and form the connected spindle cells, which can afterwards be demonstrated, by the help of special methods, in fully-developed tissue. The gradual elongation of the colourless blood-corpuscles, and subsequent thinning to the dimensions of a delicate cylindrical fibre, I have observed in the reparative stage of keratitis and in cicatrices.

All the lymph cells which undergo transformation do not become spindle cells. In a great number of them the development of a large round or oval nucleus coincides with the disappearance of all granular protoplasm in the cell, and both cell and nucleus become invisible by almost all the methods hitherto employed in histological investigation. The employment of special methods, and especially that of saturated caustic-potash solution, has during the last few years enabled us to isolate them from the adult tissue in complete layers of polygonal and long, narrow, epithelial-looking cells, in which condition they have until quite recently been hidden from observation. The changes which render them invisible coincide with the development of the fibrillary tissue, and it was hence concluded that they lost their individuality and merged in the newly-formed gelatinous substance. Their isolation from the tissue as fully-developed cells shows that this idea was founded on imperfect methods.

They had indeed been previously observed, but so changed that they could not be recognised. In tissues undergoing absorption the cells lose their vitality, and as they disintegrate become once more visible. Entangled masses of the dead cells have then been, as I have already pointed out, observed and designated "giant cells."

Lymph corpuscles develop also into stellate cells, their ramifying and anastomosing branches becoming the strong resistent fibres which, as in the cornea, for example, bind the bundles together.

The same lymph corpuscles develop into epithelium when they pass amongst the layers of previously-existing cells, or become fixed on a denuded surface adjacent to epithelial cells. The observations which I have made regarding this process, and the evidence which I have collected in support of this view, I hope to be able to publish shortly. The importance of the subject merits a fuller treatment than I can give it here.

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[1] *Loc. cit.*, 3d edition, p. 142.

One of the most striking and essential features of the reparative process which follows or accompanies inflammation, is the formation of new bloodvessels. I can here again only sketch dogmatically what I believe to be the general features of this process, and refer the reader for a more technical discussion of the subject to the *Quarterly Journal of Microscopical Science* for April 1876.

When the injury inflicted by the inflammatory process has so far weakened the bond of union between the epithelial cells, which form the tubular lining of a capillary bloodvessel, that blood plasma and corpuscles pass out between them into the lymph spaces, between the bundles of connective tissue, in some of the points in the capillary wall the stream enlarges the breach until the blood flows unimpeded into the space. This is the first important and essential step in the formation of a new vessel. The direction of the new development is determined by that of the pre-existing spaces of the tissue. The effused blood is driven by the heart's action in the direction of least resistance. But the blood, when it is in contact with tissues other than the vascular wall, and more readily when the tissues are inflamed, undergoes changes by which a coagulable material present in it forms a membranous substance on the surface of the current, which separates it from direct contact with the surfaces of the bundles. On this membranous substance the colourless blood cells adhere and develop into an epithelium. The vessel is then complete and fit for the transmission of an unaltered blood. Whether the new vessel will be permanent depends not on itself or the blood, but on changes in the tissue in which it is formed.

In the inflamed cornea I have in the walls of newly-forming vessels isolated colourless blood cells in all stages, from that of the normal appearance to a fully developed epithelial cell.

After the very first changes, the cell can only be seen with difficulty. It loses its granular appearance, and acquires a similar refractive power to that of the tissues amongst which it is placed, and can only be detected by the application of very special methods. From the wall of young vessels I have further isolated cells, in transition from colourless corpuscles to the cells of unstriped muscle.[1]

In the interfascicular spaces, the enlarged spindle cells, which I have described as one of the inflammatory changes, can be generally seen before the blood-current enters them. The escape of small rounded bodies from these cells produces an appearance of vacuolation. But this vacuolation is not a vital process connected either with the development of the cell or the growth of the vessel. In the wall of the vessel colourless blood cells elongate and form the spindle cells, which can be afterwards found as minute cellular elements, with fine connecting fibres in the developed tissue.

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[1] Arnold, quoted by Paget (*l. c.*, p. 273), found in inflammatory exudation on the free surface of the pleura, "muscular fibre-cells derived from the rounded cells of the inflammatory lymph by gradual elongation; and transitional forms, from the simple lymph corpuscle to the spindle-shaped contractile fibre-cell, were traced in the different layers of the false membrane."

The development of spindle cells into epithelium, which has been described in bloodvessels by Golobew, is not proven. I have never observed it, and as I find that the spindle cells persist as spindle cells, and that the epithelium is developed from lymph-corpuscles, I believe that it is unnecessary to conclude that there is any other mode of formation. Similar facts and arguments apply to other forms of connective tissue.

Observers who have shown that in processes of development in different organs, a diminution of size and disappearance of spindle cells coincide with the development of epithelium, have endeavoured to connect the two processes by supposing that the epithelium was produced by a change in the spindle cells. But the proof is incomplete. The transition has never been shown, and if appropriate methods are applied, I believe that it will be always possible to discover the shrunken spindle cell persisting as a spindle cell. With the disappearance of the granular protoplasm, it had ceased to be visible by the ordinary methods of observation.

The development of bloodvessels in inflamed tissue is similar to that which can be followed in fœtal and young tissue; and to make the theory I have explained more easily intelligible, I append a woodcut, which represents the formation of a capillary bloodvessel as observed in the mouse fœtus.

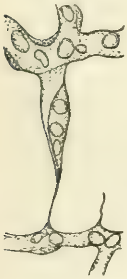

Fig. 32.—Formation of a new capillary which would have connected two vessels already formed. Mouse fœtus. Osmic-acid prep.—Magn. 300 diam.

In this figure, between the developed capillaries, a conical space is to be observed, the base being formed by the wall of the upper, and the apex by a fine line, which is continuous with the first appearance of a similar space, which is connected with the wall of the lower capillary. The large upper space is filled with a faintly granular substance above, and encroached on below by a spindle (or fusiform) cell. In this cell, there are three rounded nuclear bodies. The granular substance produced by the action of the osmic acid in the space is continuous with a similar substance within the bloodvessel, but a faint contour, less distinct in the preparation than in the woodcut, still separates the lumen of the bloodvessel from that of the space. The substance—blood plasma—which fills the space escapes from the vessel between the epithelial cells which form its lining membrane, and which by this mode of preparation are invisible. When the epithelial cells have been sufficiently displaced by the pressure of the blood current, the corpuscles will enter the space. Similar distension and entrance of blood will take place in the small space seen on the upper border of the lower capillary, and the channel between the upper and lower capillary will be completed. The organization of the wall of the new vessel will then take place. Meanwhile, as the space distends, the nuclear bodies seen in the large spindle cell escape into the space, and there present an appearance indistinguishable from blood cells. Their disappearance leaves an apparently empty space, a vacuole, in the spindle cell. What becomes of the spindle cell I do not know, as I have not traced it further; but I am disposed to believe that it disintegrates, and is absorbed. Of this, however, I have no direct evidence.

It will be well to summarize in a few sentences the general drift of the papers, of which this forms the last chapter. My object has been to show, that when a tissue is the seat of the changes which constitute the condition known as inflammation, these changes are divisible into two categories, the destructive and the reparative. The destructive changes consist, as regards the fibrillary tissue, in swelling, softening, chemical change, and disintegration; as regards the cellular elements, in division of the nuclei into small parts, swelling, death, and disintegration of the cell. The destructive change extends towards the bloodvessels, or directly implicates them; the weakened vessel permits the escape of the fluid and formed elements of the blood, and with the exhaustion or removal of the cause of the primary destruction, the process of repair begins by organization of the plasma and colourless blood cells. From the former, the ground substance (fibrillary tissue, etc.) is formed; from the latter, all the cellular elements, of what form soever. Flat cells, spindle and stellate cells, non-striped muscle cells, epithelial cells, all owe their origin to the lymph (or colourless blood) corpuscles. But the type of cell once established by the development of the original corpuscle, there is afterwards neither retrogression nor metamorphosis. The epithelial cell never again becomes or produces the colourless blood cell, nor the spindle cell develop into an epithelial cell. *Omnis cellula e cellulâ*, but not in the sense of new cells being begotten by the fixed cells of developed tissue. With the development of the free lymph cell of the fluids into a fixed element of the tissue, the power of reproduction is lost. So much, at least, I believe to be proven in regard to inflammation.

The changes which cells undergo in this process have been erroneously associated with the appearance of colourless blood cells in the spaces of the tissue. Between the one class of phenomena and the other no direct relation had ever been proved, and the results of more rigid observation have shown that none exists.

Had space permitted, I should have discussed the probable nature of the causes which, in one instance, direct the development of a lymph-corpuscle towards the form and qualities of a contractile muscle-cell, and in another of an epithelial cell. That I cannot do so is perhaps the less to be regretted, as such a discussion must have been almost entirely theoretical. In regard to epithelium, however, I may state that I have evidence that one cause of development in this form is to be found in an influence exerted by the contact or immediate juxtaposition of previously-formed epithelial cells.

I will add a few words regarding the mode of treatment I have adopted. In expounding views which, on some of the most vital points in medical science, differ from those taught by authorities whose deserved eminence has, in England at least, approached well nigh infallibility, I felt that proof must be advanced in support of every statement I made. Accordingly, in the earlier chapters, I entered into an amount of histological detail, which demanded an unusual degree of indulgent consideration on the part of readers of a journal devoted to general medicine. But this full treatment was unavoidable, as it was necessary that I should prepare the foundation for the doctrines which I have afterwards sketched. The shorter treatment, which even the space so liberally granted me has rendered necessary, when I came to consider the question of the formation of new tissue in inflammation, has compelled me to express, with almost aphoristic brevity, opinions which, in the present state of science, would require full and exhaustive discussion. I have endeavoured to supplement this omission by reference to papers which are published elsewhere. Other papers, which are now in preparation, and which are intended to fill the gaps which have been unavoidably left, will probably be published during the year as opportunity offers. In one of these, I shall, as I have already indicated, discuss at length the important question of the regeneration of epithelium. The opinion I have formed regarding its origin was held by Addison, and has been expressed since in Germany by several writers; but the criticism with which it was met by German histologists generally was short and decisive: *sehr gewagt, abenteuerlich,* are epithets applied to it by two writers, whose names have for the moment escaped me. The infancy of a science is the stage in which strong epithets are most plentiful; and judged by this standard, as by many others, histology is still in a position in which difference of opinion is permissible on any point. Holding this opinion, it has not been so much my intention to formulate in these papers a doctrine, as to indicate the results which I believe are to be obtained by the adoption of certain methods. If I have succeeded in arousing the attention of any of our younger histologists sufficiently to induce them to apply these methods for themselves, I feel sure that, whether they see reason to confirm all that I have written or not, they will not fail to contribute something towards our knowledge of the tissues in health and in disease.

I have still in my possession preparations illustrating many of the points which I have discussed, and it will give me pleasure to demonstrate them to any one interested in the subject.

ARTICLE V.—*Diphtheria: Typical Cases, in illustration of its Diagnosis, Prognosis, and Treatment; together with Remarks on the Signification of Albuminuria in that Disease.* By Sir JOHN ROSE CORMACK, K.B., F.R.S.E., M.D. Edin., M.D. Paris, Fellow of the Royal College of Physicians of London, Chevalier of the Legion of Honour, Physician to the Hertford British Hospital of Paris.

(*Continued from page* 808.)

CASE III. *Pharyngeal Diphtheria; Sudden Death a month from the first manifestation of the Disease.*

A CHILD of eleven years of age—an English girl—was treated in the summer of 1872, by another physician and myself, for severe pharyngeal diphtheria characterized by great depression of the vital powers. In this case, the false membrane never extended beyond the pharynx and tonsils.

A month had elapsed since the first manifestation of the disease: its acute symptoms had passed away, and although there still remained great debility and emaciation, with a slight and perhaps increasing difficulty in swallowing liquids, improvement in the countenance was visible from day to day. Notwithstanding the dysphagia referred to and persistent emaciation, matters seemed to be rather tending to a favourable issue under the influence of a pepsinated alimentation and the ammonio-citrate of iron and quinine taken in doses of two grains three times in the twenty-four hours. In this hopeful state of the case—a month from the beginning of the attack—the nurse was permitted, on a beautiful summer's afternoon, to carry the patient to a garden adjoining the house. The child was carried from bed to the garden, and back from the garden to bed. When in the garden, she rose from the couch on which she was lying, walked a few steps to an adjoining flower-bed, picked some flowers, and shuffled back to her couch, on which she fell back exhausted, but immediately rallied on drinking a few mouthfuls of brandy and water. When carried back to bed she was in good spirits, and seemed in all respects stronger and better than when taken into the open air. She had been laid on her bed to rest for a few minutes before undergoing the fatigue of undressing. Under these circumstances, a cup of beef-tea was brought to her: she took the cup unaided, and sat up on the bed to drink; but ere the vessel had reached her lips, she fell back in a mortal faint. I saw her lying dead on the bed an hour after she had been greatly enjoying her visit to the garden.

From the late period of the malady at which sudden death took place in this case, it is probable that the fatal syncope was dependent partly on paralysis of the heart from deficient innervation, and partly on atrophy of the muscular tissue of the organ. Under all the circumstances, it was much to be regretted that the visit to the garden had been permitted; but had the child never left her bed, she might have died in exactly the same manner from failure of the action of the heart. Be that as it may, the case affords a good example of a common mode of death in diphtheria; and shows impressively the extreme danger of allowing diphtheritic patients to quit the horizontal position even for an instant, unless fortified for the occasion by a stimulant. A remarkable ebb and flow of cardiac power had been noticed for several days, at intervals, in the case now described.

CASE IV. *Pharyngo-Laryngo-Tracheal Diphtheria; Intense Albuminuria; Rubeolar Eruption; Tracheotomy; Moderate Paralytic Affections; Recovery.*

On the morning of Friday, 24th October 1873, soon after five o'clock, a lady came to my house to request me to accompany her immediately to aid her little brother, who, she informed me, had been, just before she left home, almost carried off in a suffocative paroxysm. She said he had gone to bed the previous evening between eight and nine o'clock, suffering only from a sore throat and great debility, attributed to a diarrhœal attack of some days' duration. Till I was hurriedly summoned, the patient was not supposed to require more than domestic medical treatment. On my way to the residence of the patient I obtained from his sister most of the material facts embraced in the following history of the case:—

On Monday, 20th October 1873, E. G., an English child, aged 6½ years, returned to school after his usual Sunday at home. He did not then seem quite well, yet there was nothing which appeared to call for his being specially attended to. On the 21st and 22d, the master observed that the child frequently asked permission to leave the class for necessary purposes. On the afternoon of the 22d, permission to retire was refused, under the impression that the request was made from a mere childish trick of idleness, and not under the constraining influence of a real necessity. The sequel showed that this was a mistake. When refused, the child burst into tears, and, unable to restrain the bowels from acting, his clothes were saturated with an abundant alvine discharge. He was undressed, and his drawers being wet and dirty, he was dressed without them and sent home to his parents, that his condition might be inquired into and treated. On the 23d he complained of sore throat, breathed badly, and was observed to have nearly lost his voice. He had several liquid motions during the day. To cure the purging, he was made to drink rice-water freely; and for the other symptoms, attributed to his having caught cold in consequence of his having been sent home without his drawers, his mother administered an emetic, prescribed by a neighbouring pharmacien on her report of the symptoms. The emetic was taken just before the child went to bed. The emetic operation—violent and of short duration—afforded very considerable temporary relief to the dyspnœa, which had been becoming more and more urgent, and more and more frequently paroxysmally exacerbated, for the preceding twenty-four hours. It was about six on the morning of the 24th October, twelve hours after the action of the emetic, that I first saw the patient. I then, in reply to questions addressed to me, informed the family that my prognosis was not favourable.

The information I had received on my way to the house had excited my most unfavourable suspicions. A glance at the patient, and a few words spoken by him in reply to my salutation, confirmed my fears, and impressed me with the conviction that examination of the patient would show him to be stricken with diphtheria, already pharyngo-laryngeal, and probably advancing rapidly from bad to worse.

Before I entered, the child, as I was told, had considerably quieted from a sudden dyspnœal paroxysm. I found him supported in bed by pillows, in a semi-recumbent position. His countenance was painfully anxious in expression, and of a dirty pallid complexion. His words were spoken distinctly, but very slowly, and in a feeble husky whisper. The neck had a turgid appearance, the large veins showing conspicuously, and some of the glands on both sides, near the maxillary angle, were enlarged and tender to the touch. The pupils were responsive to light; they were abnormally dilated. From the nostrils there was a slight serous oozing, which may be described as a dampness, apparent at the usual orifices, and encroaching on the upper lip. The tongue, gums, tonsils, fauces, uvula, and the mucous membrane of the pharynx, were coated in their entire visible extent with a tough, tenacious substance of a dirty gray appearance. Having tied a handkerchief over my mouth, to prevent the possibility of my receiving any ejecta, I explored the patient's throat with the aid of a strong light. Observing a ragged edge of membrane on one of the tonsils, I took hold of it with forceps, and easily stripped off a scrap of the unmistakably diphtheritic pellicle. The pulse was very feeble, and the extremities were cold. The intelligence was unaffected. He said that the pain, which for some days had been considerable in the throat, was gone, and that his only present distress was want of air. No words were required to tell me that intense air-hunger—to Anglicise a graphic expression employed by German physicians—was the immediate source of his distress. During my visit, which lasted nearly two hours, I had ample opportunity to make this and all other necessary observations.

The treatment I adopted was the immediate administration of a wineglassful of port wine, and the application of turpentine stupes to the legs. I wrote a prescription for the glycerine of borax and the compound tincture of cinchona, both of the British Pharmacopœia, and waited their arrival. I smeared the whole of the diphtheritically-coated surfaces with the glycerate, and gave two drachms of the tincture in a second glass of port wine. Before leaving the house, I unreservedly stated the extreme gravity of the case, and in particular mentioned, that at any moment the operation of tracheotomy might be required to prevent closure of the air-passage by the rapidly-forming membranous exudation. The child was in a decidedly less alarming state, in respect to immediate danger, than when I arrived.

I returned at ten o'clock. There was not then any increased urgency in the symptoms. The mother fancied that the child's condition had ameliorated, but this opinion only rested on the fact that there had been no renewal of the dyspnœal paroxysm. A small quantity of urine, passed half-an-hour before my return, when tested by heat and nitric acid, was found to be very albuminous. Under all the circumstances, I deemed it right to repeat the unfavourable prognosis already given. I intimated to the mother and other members of the family that the child's life was in great jeopardy; that the disease was diphtheritic croup, by which the entrance of air into the lungs was being rendered more and more difficult by the air-passages becoming more and more blocked up by the formation of false membrane on its mucous surface, as well as by a submucous effusion of serosity; that at any moment the operation of tracheotomy might become imperative, and that its being rendered unnecessary by a change for the better could hardly be expected. I represented strongly the propriety of a consultation with another physician, and the despatch of a telegram to acquaint the father, temporarily absent in London, of the extreme gravity of the child's illness. The consultation was agreed to at once; but, from incredulity as to the gravity of the case, and an unwillingness to alarm an affectionate parent, the telegraphic message was not sent. Had I been absolute dictator of the course to follow, I should at this time have immediately performed tracheotomy. There was no sufficient reason for delay. Nevertheless, as the opposition to the operation on the part of the mother was very strong, and as there still remained a short period during which emetics and other measures might be watchfully though not very hopefully tried, I reluctantly consented to act energetically in another direction, and delay tracheotomy till it was sanctioned in consultation. Forthwith I administered an emetic in port wine negus. Some of this potion was swallowed, though with considerable difficulty—a small portion of it was returned through the nose. In about half-an-hour active vomiting was induced for two or three minutes, during which some shreds of tough, thick, false membrane were ejected. This was followed by a decided though very temporary amelioration, due, no doubt, to partial clearing of the air-passage and to general reaction directly caused by the operation of the emetic. The pulse became stronger, the respiration easier, and the face lost a good deal of its congested semi-purple hue. On leaving the house about eleven o'clock, I ordered a continuance of the frequent local application of glycerine of borax, and the internal use of wine and cinchona.

At six in the evening I found the child somewhat stronger, and breathing quite as easily as when I left him at eleven. There had been no suffocative paroxysm during my absence. It was agreed that my next visit should be at midnight, and that at nine next morning, Dr Benjamin Ball and I should meet in consultation. I directed that there should be a continuance till midnight of the treatment which I had laid down at my forenoon visit.

At midnight, I found that the temporary amelioration had ceased, and that the symptoms were even more alarming than I had seen them. The child had a semi-asphyxiated appearance, and breathed with great difficulty. The face was swollen and purple, and the neck was turgid. I sent a messenger to summon Dr Ball, as it seemed very doubtful whether the operation could be delayed till nine next morning, the hour for which our consultation had been fixed. When Dr Ball arrived, the patient was breathing much better. The only explanation of this change which suggested itself to us was that, in the terrible struggle for breath, portions of the laryngeal false membrane had become detached and displaced in such a way as to allow a freer admission of air into the lungs. After an anxious consultation, Dr Ball and I came to the conclusion that, as the family were exceedingly opposed to tracheotomy, and as there was a remission in the symptoms, we might delay the operation yet a little longer.

Saturday, 25th October.—At nine A.M. Dr Ball and I met in consultation. We urged the operation as an immediate necessity, but without success. There being no increase in the urgency of the symptoms, we agreed to delay, and to meet again at nine in the evening to decide the question. Several times during the day, I paid short passing visits, and on each occasion found matters getting worse and worse. At nine P.M. when Dr Ball and I met, we felt that there was no time to lose ; the child was becoming asphyxiated. We insisted on immediate operation, or, if time permitted, a consultation with some eminent authority in such cases. We suggested that Dr Jules Simon should be summoned at once, and before ten o'clock we were in consultation with him at the patient's bedside. He thought that the operation had been too long delayed, and strongly expressed himself to that effect to the mother, who, looking at her child, then fearfully struggling for air, fully surrendered him to us. We told her that, without immediate tracheotomy, the child must inevitably die within less than an hour—perhaps much sooner; and that he might be saved by the operation. We also said that, on the other hand, he might die ere the operation were completed. In so speaking we did not exaggerate the gravity of the situation.

Dr Jules Simon operated at ten P.M. There was an immense flow of blood during the operation from the congested veins. The wound being freely held open for half a minute, large portions of false membrane were forcibly expelled by the expired air, and other large shreds were drawn out of the trachea by the forceps. Immediately before, and also during the operation, a favourable issue seemed very improbable. No sooner, however, was the tube inserted, than the loss of blood ceased, and the symptoms of asphyxia disappeared. After the administration of some port wine negus, the child fell asleep. I remained with him. He awoke in an hour, from portions of detached false membrane choking up the tube, and thereby inducing a momentary return of the asphyxial symptoms. Without taking the tube out of the trachea, I was able to clean it perfectly by dragging out large pieces of membrane. I remained with the child from the time of the operation till seven next morning. During the night he slept a good deal, and took a fair amount of beef-tea, alternated with port wine negus, to which latter a teaspoonful of the compound tincture of cinchona (*Brit. Ph.*) was twice or thrice added.

Sunday, 26th October, one P.M.—Drs Ball, Simon, and I, met in consultation. There was some subcutaneous emphysema around the operation-wound. A bright rubeolar eruption was conspicuous on the trunk and on the legs. This eruption had been faintly visible in the morning. The urine was intensely albuminous; it became a semi-solid mass when treated with heat and nitric acid. The pulse was 120 ; the temperature was 37·8° Cent. Shreds of false membrane were at this time being constantly got rid of through the tube. A mixture of perchlorate of potash and tincture of the chloride of iron was prescribed. Very little of this mixture was ever taken, the patient's mother objecting to ferruginous medication, which she said did not agree with her family.

Monday, 27th October.—At my first visit (seven A.M.) I found the condition not quite so favourable. There had been some oozing of blood from the operation-wound, the edges of which were coated with diphtheritic pellicle. The urine was intensely albuminous. The pulse was 120, very feeble. The temperature was 37·8° Cent. At noon the state was very similar. At ten P.M. there was a still feebler pulse. The greatest difficulty had been experienced during the day in administering aliment. He was evidently in urgent want of his beef-tea, and yet he would not, or could not, swallow a drop of either of them. Under these circumstances I resolved to remain with him some hours of the night. Forthwith, an enema of beef-tea and brandy was administered to him. I left him for nearly two hours. On my return he had another similar enema.

Tuesday, 28th October.—About three in the morning, I was slumbering on a sofa which commanded a full view of the patient, when I was aroused by the cries of his attendants, and a strange noise proceeding from the patient. He was cold, comatose, and convulsed. In an instant I withdrew the canula, which was choked up with mucosity and membranous shreds. I kept the wound dilated, whereupon respiration was re-established, and a large quantity of frothy mucus expelled. The canula was then replaced. The breathing was so feeble, the pulse so weak, and the surface so cold, that I feared the worst. A large cataplasm of mustard and linseed to the chest, turpentine stupes to the legs, and an enema of beef-tea and brandy were all quickly resorted to. In about an hour or an hour and a half from the time I was aroused, Dr Jules Simon (whom I had at once sent for) arrived. The child had then very nearly recovered from the critical accident which has now been described. The day passed without the occurrence of any noteworthy incident, except that once the tube was taken out and cleared of the mucosity by which it had again become clogged. At five P.M. the temperature was 38° Cent. (100·4° F.). The urine was still very albuminous, though decidedly less albuminous than it had been since the 26th.

Wednesday, 29th October.—In the morning an amelioration in the state of the patient was observable. The pharynx, nostrils, and operation-wound were, for the first time since the operation, free from the diphtheritic pellicle. The cervical emphysema and the rubeolar eruption had both disappeared. For an hour the canula was withdrawn. For the last two days the patient had taken an abundant and varied supply of stimulants and aliment, consisting of champagne, brandy, beef-tea, and pounded raw meat with its juice. He had, however, no desire for food. There was great wasting of the body. At noon the pulse was 96, and the temperature 37·7° Cent. (99·8° F.). There was observed at the same hour a notable diminution in the quantity of albumen in the urine. The urine examined was passed at ten A.M. At my subsequent visits, at seven and eleven P.M., I found that there was a return of the fever. At seven o'clock the pulse was 124, and the temperature was 38·8° C. (102° F.). Some urine passed about ten P.M. was much more albuminous than that passed at ten A.M.

Thursday, 30th October.—The patient passed a tranquil night. This forenoon he remained without the canula for more than an hour. During the day the breathing became oppressed; and, on examining the chest, the respiratory murmur in the right lung was found to have become feebler. At nine P.M. I found that the condition had somewhat improved since the morning. The dyspnœa and feebleness of respiratory murmur in the right side had not increased. The urine passed during my visit was rather less albuminous than some which had been passed five hours previously, and kept in a separate vessel. The pulse had fallen from 120 to 104, and the temperature was exactly the same as in the morning —viz., 38·8° C.

Friday, 31*st October*.—He passed a good night. At one P.M. the pulse was 104, and the temperature was 38·8° C. (100·4° F.). During the greater part of the day he remained without the canula. He was stronger. The alimentation had been diligently and successfully carried out. In the right lung the respiratory murmur was very coarse.

Saturday, 1*st November*.—He passed a good night. He was the whole day, except an hour, without the canula. The breath-sound was still rude in the right lung, but there was a return of air to the smaller tubes. There was a considerable sero-sanguinolent flow through the operation-wound, the edges of which had a healthy appearance.

Sunday, 2*d November*.—The night was disturbed, but nevertheless his condition had improved. The pulse was 88, and firmer; the temperature was 37° C. (98·6° F.). The right lung was much less engorged. The operation-wound was contracting. The child breathed through the larynx and the nose. The urine was still highly albuminous; but the quantity of albumen it contained was now insignificant, when compared to that which it contained on the 26th October.

3*d*, 4*th*, *and* 5*th November*.—These were three days of progressive amendment, during which the pulse varied between 84 and 88, and the temperature (often taken) was never found above 37·8° C. (100° F.), and was generally about 37·2° C. (99° F.). The urine, tested twice or thrice in the twenty-four hours, contained a constantly diminishing amount of albumen till the afternoon of the 5th, when not even a trace could be detected. During these three days the child was without the canula.

6*th November*.—The improvement continued. The child slept well and much. He took abundance of suitable food with appetite. He could blow out a candle, and speak in a low voice. The operation-wound was very nearly closed.

7*th November*.—For the first time, the child left his bed, and took his meals sitting at a table beside the fire.

1*st December*.—The child being nearly well, daily attendance has lately been discontinued. He speaks in a natural manner. The tracheal wound has solidly closed, and the strength is steadily returning.

The medical and hygienic treatment after the operation consisted in good varied alimentation, pepsine being given with the food, a liberal employment of wine and other alcoholic stimulants; and the maintenance day and night of an equal temperature in the room; the atmosphere of which was moistened by steam from a tin basin of water, below which a spirit-lamp was kept burning. I ought to add, that the room was spacious, and the house situated in an airy locality adjoining the Champs Elysées.

24th December.—The child had now regained sufficient health and strength to walk out, sleep, and eat very much as before his illness. He was, however, very thin. About 20th November, he was observed to be weak and languid, but in no way paralyzed. On 24th November, about five weeks after the operation, paralytic symptoms were manifest. The child had a curiously restrained, or rather contracted appearance, in consequence of the head falling forwards, and the head being twisted. There was internal strabismus of the left eye. By an effort of the will, he could raise his head, but was unable to correct the squint. The pupils were equal. The third and fourth pairs of nerves were not affected; the sixth pair only were paralyzed. The power of accommodation was lost. He had no difficulty in swallowing, nor had he any paralytic symptoms whatever, except those now described. By the advice of Dr Galezowski, a solution of eserine was dropped into the eye. Ferruginous tonic treatment, which had already been resumed, was continued. After fifteen days, the paralytic symptoms which I have described had entirely disappeared, and no form of paralysis afterwards manifested itself.

The patient was in perfect health in November 1874—twelve months after the operation of tracheotomy saved him from impending suffocation.

The case now described was a severe and a typical case of diphtheria, terminating in recovery, after death had been averted by tracheotomy, which was imperatively demanded by the almost total exclusion of air from the lungs by the laryngo-tracheal exudation. Tracheotomy saved the child, but did not cure his diphtheria: it enabled him to breathe—to live—whilst Nature, judiciously and unceasingly assisted by Art, accomplished a cure.

(*To be continued.*)

ARTICLE VI.—*Case of Absence of Uterus, with a Tumour of Doubtful Character in each Inguinal Canal.* By Dr C. E. UNDERHILL.

(*Read before the Obstetrical Society of Edinburgh, 9th February.*)

THE following case was observed in the Royal Infirmary, in September 1874, when I had charge for a short time of Dr Matthews Duncan's Ward, and by his permission I will read the account of it from the case-book, and then draw attention to one or two points of interest in connexion with it:—Jane V., aged twenty, single, residing in Fife, was admitted on 23d September 1874, and examined the following day. She complained of never having menstruated, and of an eruption which, she states, breaks out on the arms and legs about every six weeks, continues for about a fortnight, and then disappears. About once a fortnight she feels sick and loses consciousness for a few moments. (The eruption was not visible, and she had no attack of unconsciousness whilst under observation.) These symptoms commenced about three years ago; otherwise she enjoys good health. She has one sister who is married and has three children, and two other sisters who are younger than herself and who menstruate regularly. She has no menstrual molimen. The patient was five feet two inches in height and of well-developed female shape, with a fine bust, a good-looking face, and a considerable quantity of dark hair. Her voice was rough. The breasts were well formed and of average size, the nipple being indistinct and small. On examination per hypogastrium the belly was natural and moderately fat. Lying close to the external abdominal ring on each side, was an oval tumour, about the size and shape of a testicle, the length of the oval being in the course of the inguinal canal. These tumours were freely movable. The anterior end of each tumour was hard; the posterior much softer—the left one rather larger than the right. They were not tender or even sensitive to the touch. She only noticed them about a year ago. The hair of the pubes was scanty and growing towards the groins. The mons Veneris was inconspicuous. The labia majora were thin, flat, and small, with a very small amount of fat in them. The clitoris and nymphæ were small. A vaginal examination showed that there was no hymeneal obstruction or tenderness. The vagina, which was smooth and not tight, was between two or three inches in length, and terminated in a cul-de-sac, in which no trace could be felt of the os or cervix uteri. The parts were loose and easily examined, but nothing resembling uterus or ovaries could be made out by bimanual examination or by examination per rectum. A sound was then passed into the bladder and was freely movable, and its point could be pushed down behind the extremity of the vagina without encountering any resisting body. Combined examination by means of the finger in the rectum and the sound in the bladder failed to detect any representative of the uterus. The pelvis was large and broad, and apparently well formed on a female type, the measurements across the iliac spines being eleven inches, and from crest to crest twelve and a half inches. The patient stated that she felt sexual desires, and one reason for her visit to the Infirmary was for advice with regard to marriage.

Here, then, was a woman in all other respects large and well formed, except in the organs of generation, of which the external were small and ill formed, and the internal either wanting or of doubtful character. At the time I saw her I looked upon the case as one in which the uterus was either absent or reduced to a mere fibrous point, complicated with prolapsus of both the ovaries into the inguinal canals, a combination of which I have not found any example verified by post-mortem examination. But I was much struck by the resemblance which the tumours in the groins bore to testicles. They were of considerable hardness at the lower and smaller end, and were larger and softer at their upper and posterior part, giving one quite the impression of testicles and epididymis. I unfortunately did not specially examine for any structures resembling vasa deferentia passing down into the pelvis, nor did I notice anything resembling them in the vaginal examination.

Cases of absent or imperfectly developed uterus have been recorded by numerous authors, and have been found usually, though not always, to be attended with some other defect in the formation of the sexual apparatus.

In many of them the vaginal canal is altogether wanting, but sometimes a rudimentary vagina is present, ending in a cul-de-sac, while no other organs are to be made out. I had an opportunity of seeing a case of this kind some years ago, which Dr Croom asked me to see with him. The patient, aged about thirty, had been married several years to a soldier, but had never menstruated, and sexual intercourse could only take place in a very imperfect manner. The general formation of the body was good, and the external genital organs were well formed, but the vagina was found to be not more than an inch in length, and to terminate in a blind extremity, beyond which nothing could be felt. Combined examination by the rectum and bladder failed to show any evidence of the presence of a uterus. She had no sexual desire.

Kussmaul,[1] whose work on the absence and imperfect development of the uterus embraces almost all that is known on the subject, relates many instances of absent and imperfect uterus, but none of them show the presence of ovaries in or near the inguinal ring, except the very remarkable case of Steglehner, which I shall relate further on. After a review of the whole literature of the subject, he summarizes his opinions in a few propositions, of which I will mention such as seem to have a bearing on the case I have just related. Entire absence of the uterus, or its appearance as a mere fibrous thread, apart from the malformation of monsters, is exceedingly rare. In the living woman absence of uterus cannot with certainty be recognised. Great care is required to avoid confounding these cases with male hemaphrodites, having the external organs and formation of the female along with misplaced testicles. (To this remark, I shall recall your attention presently.) The best guide for recognising a rudimentary uterine horn from a Fallopian tube or ovary is the round ligament, whose place of insertion always shows the place where the uterine horn has its external extremity. The ovaries are sometimes present, sometimes not: in the first case, they may contain many well-developed Graafian follicles; in the second case, one has to do, strictly speaking, with sexless individuals. Such a condition, I may mention, was found by Dr Hauff[1] in 1873, who made an autopsy on a woman, aged 51, in whom there were no traces of even a rudimentary uterus or ovaries; the vagina was present, and ended in a cul-de-sac; she was otherwise a handsome, well-made woman, with well-made pelvis; the external genitals were undeveloped and without hair; she was said to have had sexual desires. The same condition of parts existed in two nieces of this woman.

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[1] Kussmaul, Von dem Mangel der Gebärmutter. Chap. III. Wurtzburg, 1859.

The broad ligament appears to be wanting in complete absence of the internal genital parts, bladder and rectum being bound together as in the male. When ovaries and tubes are present it has its place on either side of the bladder, and the spot where the uterus ought to be is often shown, by a thicker layer of connective tissue mixed in the muscular fibre cells, to be between the bladder and rectum.

In all cases hitherto reported, the vagina was either altogether wanting or ended in a blind extremity; the external organs of generation may be normal, while the internal are wanting, and *vice versâ*. The pelvis may in exceptional cases of absence of uterus be broad, and even possess the female dimensions. The breasts are generally found well developed in these cases. The form of the entire body has been found, even when uterus and all internal genital organs were wanting, to be of the female type, as have also the voice, tastes, and temper. Menstruation, and as a rule the menstrual molimen, are entirely wanting, according to the express description of the majority of observers, even when ovaries containing numerous Graafian follicles were present.

The other element in the case—namely, hernia of the ovaries—seems to be a much rarer condition, though when it does occur it is not unfrequently on both sides at once; it may be simply a descent of the ovary, or the hernia may include uterus and Fallopian tubes, intestine, etc. In the 38 cases collected by Engleisch,[2] in which the ovary alone descended, 27 were inguinal, 9 femoral, 1 sciatic, and 1 obturator. In one-third of the inguinal cases, it was on both sides, and there were no double cases, except in the inguinal region. Ovarian hernia is frequently, he says, combined with other malformations of the genital organs. It is rarely that any information as to diagnosis can be obtained from the form and consistence of the swelling, which is usually pear-shaped, with a very narrow neck. Ovaries in this position are frequently inflamed, and sometimes give rise to great pain on sexual intercourse—so much so, that in one case of double hernia, observed by Biegel,[3] it had to be altogether given up. In his other case, where both ovaries had been down since the woman was twelve years old, there was no such trouble, and she had had several children. They swell during menstruation, and in some instances also during sexual intercourse. Whether this swelling is painful or not appears to depend on the size of the hernial aperture. The healthy prolapsed ovary has been on several occasions removed, as in the classical case of Mr Pott,[1] who took both ovaries from a young woman aged 23, in consequence of the pain and trouble they gave her at her work; and more lately,[2] Dr Meadows has removed the ovary on one side for a similar reason. Both these cases were successful. But Engleisch states that one half of the cases in which extirpation of a healthy irreducible ovary was performed died of sub-peritoneal inflammation and its results. In the case which I related above, there was no pain or tenderness in connexion with the prolapsed organs, nor did they give rise to any sort of inconvenience.

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[1] See Syd. Soc. Biennial Retrospect, 1873-74, p. 389.

[2] Jährbüch der k. k. Gesellschaft der Aertze, in Wien, 1871. Quoted in Syd. Soc. Year-book, 1871-72, p. 291.

[3] Die Krankheiten des Weiblichen Geschlects. Erlangen, 1874. Vol. i. p. 436.

Verdier[3] mentions a case of inguinal ovarian hernia of the right side. The size of the swelling was variable, but it became enlarged before menstruation, and considerably smaller when the period was over.

Meissner[4] also speaks of this enlargement coincidently on the menstruation, and quotes Churchill in confirmation of the fact, but considers that it is not so invariably and certainly present as to be of much value as a diagnostic sign.

The case most nearly approaching the one I have related above is that of Oldham,[5] who frequently had under observation a person in whom both uterus and vagina were wanting, though the pelvis was of a well-formed female type, and the secondary sexual characteristics were feminine. The patient had a swelling on each groin. That in the right groin appeared first in her eighteenth year, caused a little pain, and then disappeared; reappeared in a few months, and again disappeared. When seen by Dr Oldham it was the size of a goose-egg, and extended from the external abdominal ring to the middle of the labium; the parts covering it were œdematous and inflamed. He continues, "A more critical examination detected the presence of a solid body of an oval shape within the tissue of the labium, which proved to be the ovarium." On the left side was another oval body, the size of a walnut, which passed just beyond the outer ring, but slipped readily into the canal. Eventually both these tumours enlarged at intervals, which at length occurred every three or four weeks. They continued to swell for four days, remained stationary for three days, and then slowly subsided. There were no sympathetic pains in the mammary glands or elsewhere. The patient was subsequently lost sight of. In a second case related by

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[1] See Cyc. of Anat. and Phys., vol. v. p. 573.

[2] Obstet. Soc. Trans., vol. iii. p. 438.

[3] Traité des Hernies, p. 396, 1840.

[4] Frauenkrankheiten, vol. ii. p. 343.

[5] Proc. Roy. Soc., vol. viii. p. 377.

Oldham, a person, well formed on the female type, was found to have an oval tumour about the size of a chestnut, which he assumes was an ovary, in each inguinal canal; but they did not swell or give any trouble. They were easily reduced by pressure. This patient had no uterus or vagina.

In my case no such swelling or menstrual molimina were present—an important point in the diagnosis, though Engleisch states that they have not occurred usually in cases where the uterus was absent.

I have so far looked upon the case as if the tumours which were found in the inguinar legions were certainly ovaries, but this is open to serious question. In a paper recently published by Leopold of Leipsic,[1] a very similar case is described, in which he considers that, though the external sexual organs were those of the female, and the general type was feminine, in reality the tumours in the groins were testicles, and the person was to be looked on as an example of a rare form of hermaphroditism, that called in the classifications, "transverse hermaphroditism, with the external parts female." The malformation consists in the presence of the external organs of generation on the female type, while the internal and essential organs are male. Only three or four of these cases have been hitherto described in the human subject, but in the lower animals they are not so uncommon—the so-called free-martin cow being an example of this condition, which has also been observed by Sir E. Home, Rysch, and others, in the horse, dog, and sheep.[2] The case of Leopold probably belongs to this class, though not verified by post-mortem examination. A woman, aged 50, who had been twenty-five years married, came under his care with cancer of the liver. She had had no child, and had never menstruated or had menstrual molimina, but otherwise was healthy. On vaginal examination the labia majora were found small and thin, and but sparely covered with hair; the clitoris and nymphæ small. The vagina, small, 2½ inches long, ended in a cul-de-sac. No solid substance could be made out either by vaginal or rectal examination, combined with a catheter in the bladder. At the upper end of the vagina, and between it and the rectum, was felt a semicircular fibrous band, lying across the pelvis (this was considered to be a thickening of the peritoneum in Douglas's space, due to old exudation); it was smaller and thinner at its extremities than in the middle. The measurements of the pelvis corresponded to the female standard—Spines, 10¼ inches; crests, 11½; external conjugate, 7¾; promontory not to be reached by the examining finger. In the groins were found two almond-shaped, roundish bodies, of the size of chestnuts, freely movable, which lay hidden in the upper part of the labia majora, and just below the crest of the pubes. Several fine cords could be felt passing from them over the pubic arch, there uniting to form one cord, and descending into the pelvis. These bodies he looked upon as testicles, because double-sided hernia of the ovaries is more commonly found in cases with otherwise normal sexual organs, because there had never been any menstrual discharge or molimina, and because the cords passing into the pelvis had the feel and relations of vasa deferentia.

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[1] Archiv für Gynakologie, Band viii., Heft. iii., S. 487.

[2] Cyc. of Anat. and Physiol., vol. ii. p. 703.

This case, it will be seen, very closely resembles mine, and also that of Oldham, excepting in the absence of the periodical swelling which took place in the latter case; but this also happened in the very remarkable instance published by Steglehner in 1817.[1] A good-looking and well-born girl, 23 years of age, had a well-developed, though small, vulva, clitoris, and nymphæ; hair scanty, and a very narrow vagina, half shut up by a hymen. She died of tubercular disease of the lungs. Steglehner could find no uterus, Fallopian tubes, or ovaries. On further examination a testicle, with its spermatic cord, was found in each inguinal region, placed outside the external ring, and surrounded with its cremaster muscles and vaginal coats. The testicles were flaccid and small, but their internal structure, and that of the epididymis, was natural, and the slender vasa deferentia passing from them descended into the pelvis, and were joined behind the urinary bladder by two vesiculæ seminales of considerable size. Their common ejaculatory ducts opened into the vagina. The form of the thorax and body in general was feminine, and the mammæ and nipples were well developed. During life the voice approached that of a man in tone; and though menstruation had never taken place, menstrual molimina had occurred at tolerably regular intervals. How these molimina showed themselves is not mentioned in the abbreviated accounts I have read of the case.

Two other instances are related by Leopold in his paper as being all that he can find of this form of hermaphrodite. That published by Ricco was as perfect an example of the condition as the last. The malformation was found in a person aged 80, who had passed through life as a female, and had been married as such. On section, the external organs of generation were those of the normal female, except that there were no vestiges of hymen or carunculæ myrtiformes. The canal of the vagina was two inches long, without rugæ, and ended in a cul-de-sac. There were no uterus, ovaries, or Fallopian tubes. Testicles were found of the usual shape and size in the region of the pubis, close to the inguinal rings. They had internally the structure of tubuli seminiferi imperfectly developed. Vasa deferentia led to vesiculæ seminales, lying between the bladder and the vagina. The shape and dimensions of the pelvis were those of the male type.

The subject in Giraud's case was also married, and had lived for several years in intercourse with a husband. She presented a mixture of the secondary sexual characteristics, having the broad pelvis and fine limbs of a woman, with the voice and beard of a man. The sexual parts were all masculine, except an imperfect vagina and vulva; indeed, this case is related by Sir James Simpson, in the "Cyclopædia of Anatomy and Physiology," under the heading of "Pseudo-Hermaphroditism."

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[1] *Vide* Leopold's paper, and Cyclop. of Anat. and Phys., vol. v. p. 703.

To the three cases related by Leopold, I have been able to add a fourth, which is mentioned in a paper on Hermaphroditism by Mr Durham.[1] The subject was a child, age not mentioned, which had during life been considered as hermaphrodite, but turned out on dissection to be a male. The external organs were those of the female; clitoris rather large for so young a child, but presenting no further resemblance to a penis; nymphæ normal. Below the clitoris was a canal, into which opened the meatus urinarius, and a canal which must be regarded as representing the vagina. It was a quarter of an inch in diameter, and led to a dilated oval pouch, lying between the rectum and bladder. This pouch, he thinks, may be taken to represent either uterus or prostate. All the other organs were male. In the upper part of each labium was a testicle, as proved by microscopic examination. Vasa deferentia small, but taking their usual course. Vesiculæ seminales present, but small. No ovaries or Fallopian tubes.

The question to be solved, then, in relation to my case is, are we to look upon it as one of undeveloped or absent uterus, with hernia of the ovaries, at first sight the more probable hypothesis, or are we to consider it as allied to the five cases just related, and to be an illustration of the transverse external hermaphrodite? To put the matter more simply, are the tumours in the groins ovaries, or testicles? The latter view is supported by the facts that the tumours had entirely the feel of testicles; and this was noted when I never doubted that I had to do with an imperfectly-formed woman. They were not tender; they were freely movable; their shape was distinct—points all differing from the general conditions observed in hernia of the ovaries. The external genitals were ill-developed; the labia thin, hair scanty, uterus absent, vagina smooth; no menstruation or menstrual molimina. Very close similarity to the cases of Steglehner, Ricco, and Giraud—of whom two had lived all their lives as women, and had been married many years, and had yet turned out, on examination after death, to be men; and to the case of Leopold, which, from want of the post-mortem inspection, cannot be claimed as a certain example of the condition, though the structures representing vasa deferentia which were found render this view highly probable. On the other hand, the whole appearance and form of Jane V. were those of a woman. The pelvis was of feminine type, and capacious. External organs, vagina, and breasts pointed in a similar direction, and this view is supported by those rare cases mentioned above, where absent uterus was found combined with apparently prolapsed ovaries. Without an autopsy the matter cannot be definitely settled, but I incline on the whole to the hermaphrodite theory.

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[1] Guy's Hosp. Rep., 1860, p. 428.

ARTICLE VII.—*On the Reflex Functions of the Spinal Cord.* By WILLIAM STIRLING, D.Sc., M.D., Demonstrator of Practical Physiology in the University of Edinburgh.

NERVOUS physiology was founded in England. John Hunter was the first who, with few and masterly touches, foreshadowed the plan for the construction of the new doctrine. His distinguished successor, Sir Charles Bell, from observing the course of the nerves, concluded that the nerve roots had different functions. In 1811 he published "An Idea of the New Anatomy of the Brain, submitted for the observation of the author's friends." Therewith were now the channels by which it was possible to approach the goal—the relation of sensation to motion. But long before this, Robert Whytt had, to a certain extent, explained the old obscure doctrine of "Sympathy," according to which the sympathetic was the medium of nervous communication between different parts of the body.[1]

Several decades later, Prochaska introduced into physiology the notion and name of reflex action. He says, "Impressiones externæ quæ in nervos sensorios fiunt, per totam eorum longitudinem celerrime ad origionem usque propaguntur; quo ubi pervenerunt reflectuntur certa lege, et in certos ac respondentes nervos motorios transeunt, per quos iterum celerrime usque ad musculos propagatæ motus certos ac determinatos excitant. Hic locus in quo tanquam centro nervi tam sensui quam motui dicati concurrunt, ac communicant, et in quo impressiones nervorum sensorium reflectuntur in nervos motorios vocatur termino plerisque physiologis jam recepto sensorium commune."[2]

Emil du Bois Reymond[3] showed, that even one hundred and fifty years before Prochaska, Descartes had known the art and meaning of reflex movements. The example used by René Descartes[4]—the closing of the eyelids when an object is brought near to the eye—for explaining the principle of reflex action is (together with other examples) cited, even literally translated, by Prochaska.[1]

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[1] An Essay on the Vital and other Involuntary Motions of Animals, Edinburgh, 1751. R. Whytt was Professor of Medicine in the University of Edinburgh, and seems to have been one of the most acute thinkers and exact reasoners of his time. Unzer, in the preface to his great work on physiology, "Erste Gründe einer Physiologie," von D. J. A. Unzer, Leipsig, 1771, says, "Modern physiologists whose names Europe knows and honours—Haller, Zimmermann, Whytt, and Œder—have rendered much service to this department of physiology." An excellent account of the life and writings of Whytt is given by Dr Wm. Seller, in the Transactions of the Royal Society of Edinburgh, vol. xxiii. part 1, p. 99.

[2] G. Prochaska, "Operum Minorum Anatomica Physiologici et Pathologici Argumenti," pars ii. p. 150. Viennæ, 1800.

[3] Gedächtnissrede auf Joh. Müller, Abhandl. der K. Akad. der Wissensch. zu Berlin, 1859, p. 79.

[4] Les Passions de l'Ame, p. 53. Paris, 1849.

Prochaska, in opposition to Whytt, had made considerable progress in the knowledge of the functions of the central nervous system. Whilst the latter regarded the soul as distributed over the whole body,[2] the former separated the "sensorium commune" from the soul.

The division of the soul is carried to an extreme extent by Légallois.[3] He says, "If instead of destroying the spinal cord you make several transverse sections, the parts corresponding to each segment of the cord possess the power of sensation and voluntary movement, but without there being any harmony, and in a manner independent of each other, just as if you had cut the body of the animal itself at the same height. In one word, in this case there are constantly as many distinct centres of sensation as there are sections of the cord."

To these words of the posthumous work the Secretary of the Paris Acadamy adds—"This movement is caused by a will which has nothing of the intellectual. A will, an internal impulse, alone produces these movements. In the present case the movements, though voluntary, are irregular; there is no co-ordination between them."

Lallemand had confirmed this view of Prochaska and Légallois, by observations on anencephalous fœtuses. He says, "These observations are sufficient to prove that the brain is not the only source of nervous power, as Haller believed—the sole centre of the nervous system of animal life, as Bichat imagined. Respiration, deglutition, sensibility, and movement have all existed in these fœtuses in spite of the absence of the cerebrum and cerebellum. No objections can be brought against the conclusion, that these functions are independent of those organs, so that, consequently, the medulla oblongata and the spinal cord do not derive, either from the cerebrum or cerebellum, the nervous power which animates the parts receiving nerves from them."

Results similar in principle had been observed by Gilbert Blane.[4] After decapitating a kitten and dividing its spinal cord between the lumbar vertebræ and the sacrum, he found that the posterior extremities were incapable of movement. The tail, however, could move. From this it might be urged, that the portion of the spinal cord below the section had served as a sensorium for this part. To this, however, the author replies, that the capacity for movement also continued in the head, as was shown by the condition of the ear upon pinching and touching it with a hot wire. Here consciousness and sensation must be present in the parts of the body separated from each other. An anencephalous fœtus exhibited similar phenomena. It drew up its legs when the soles of its feet were tickled, could suck, expelled urine and fæces, and swallowed food. These facts clearly show that instinctive or rather automatic movements can take place without the intervention of the sensorium commune, and therefore without consciousness and sensation.

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[1] *Loc. cit.*, p. 155.

[2] Works, p. 288. Annotation.

[3] Œuvres, tome i. p. 135. Paris, 1830.

[4] Select Dissertations on several Subjects of Medical Science, 1788. P. 262.

Calmeil,[1] after a series of experiments, arrived at results similar to Légallois. Following the teaching of Cuvier, he regarded the brain as the place where the sensations took a distinct form, left traces and permanent recollections, and says, "The spinal cord of reptiles, of young birds, and young mammals appears equally susceptible, after removal of the brain, of being modified by our irritations, of feeling them, and, consequently, of controlling projected movements, and which are not to be confounded with the convulsive and evanescent twitchings due to irritability." A similar view was supported by J. Bostock.[2] P. Flourens[3] went into the opposite extreme from Légallois regarding the seat of the intellect, consciousness, and sensation. Longet,[4] in his great work on Physiology, expresses himself much more guardedly respecting the functions of the spinal cord. He says, "There is no doubt that the spinal cord is not like a nerve, simply an organ of transmission, but that it is rather a centre of independent action. In fact, one sees that, notwithstanding the absence of the encephalon, it may still be the seat of the extremely remarkable phenomenon of "centralité" (however that expression is to be interpreted).

Regarding such a view, to which Volkmann also leaned,[5] Johannes Müller remarks,[6] "It is indubitable that in many cases reflex phenomena may occur (with intact brain and spinal cord) in a purposive manner. Thus I regard, for example, the closure of the eyes under the influence of a strong light, and the action of the respiratory muscles induced by irritation of the mucous membranes of the respiratory organs, intestinal canal, or urinary system. But when we remember that, if we divide the trunk of a Salamandra maculata into several portions, each part, if it contain a fragment of the spinal cord, will still evince the reflex motion, we can scarcely maintain the applicability of this view to all cases. The reflex phenomena are observed also to occur in parts withdrawn from the influence of the will, such as the intestinal canal and heart. The general spasms, lastly, which are excited by stimulus of a sensory nerve in animals in a state of narcotization, do not in the slightest degree resemble the phenomena of spontaneous reaction."

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[1] Recherches sur la Structure, les Fonctions, et le Ramollissement de la Moelle Epinière, Journal des Progrès, tom. xi. p. 87. 1828.

[2] An Elementary System of Physiology, vol. i. p. 263. London, 1824.

[3] Recherches experiment. sur les Propriétés et les Fonctions du Système Nerv. dans les Animaux Vertèbres. 1st edition. Paris, 1824.

[4] Anatomie et Physiologie du Système Nerv., p. 307. Paris, 1842.

[5] Müller's Arch., p. 22. 1838

[6] Handbuch den Physiol. des Menschen. Coblentz, 1834. Vol. i. p. 621. Translated by Baly. London, 1840, p. 767.

"The slightest local sensation is here changed into a movement, not only of the corresponding muscles, but also of the entire body, *i.e.*, co-ordination. Reflex movements are combined to produce a result not only against the will, but completely purposeless. All extensive injuries cause frogs to pass into the highest reflex, and indeed purposeless, tension of the spinal cord, which in the frog, whose liver I excised, occurred on the third or fourth day. The animals sat quite quiet; but on the slightest movement, they drew themselves together. If they were preserved from all irritation they could be kept for a time; if, however, I seized them with the hands, instantly they became tetanic; and although previously they had sufficient power to sit erect, in a few seconds they were dead."

According to Müller's view, the stimulation causes at first a centripetal action of the nervous principle in the sensory spinal nerves to the spinal cord. If this arrives at the sensorium commune it becomes conscious sensation. If, however, on account of section of the spinal cord it does not arrive at the sensorium, then it retains its whole energy for reflex action or centripetal reflexion. Mayo[1] had already found that not only did the iris contract when light fell upon the retina, but also when the cerebral end of the divided optic nerve itself was stimulated, *i.e.*, an apparently purposeless reflex action through the intervention of the parts of the brain. But by these already very clear deductions, the views of physiologists were by no means brought into unison. Volkmann had changed his views, for, in the year 1838, he says, "The known facts are not sufficient to prove that all the reflex movements of decapitated animals, and especially of decapitated amphibians, occur with the co-operation of the soul, as the principle of sensation and will." In the year 1844, on the contrary, he expressly distinguishes the reflex from the voluntary movements.[2] "In spite of the extraordinary purposiveness of the former, which not unfrequently are accompanied by accidental conditions, there is in the process of reflex movement such an amount of regularity as does not permit of the assumption of a psychical element."[3] At the same time, he expresses himself against Arnold's doctrine of reflex action,[4] which requires an unconscious soul, and will not allow the transference from sensory to motor nerves—a view also endorsed by Spies. Spies regards the isolated conduction in all fibres of the nervous system as the fundamental law of animal organization.[5] Now, as the motor nerves are stimulated from the brain, and the sensory ones conduct their impressions thither, then, for reflex movements, the existence of a special set of anastomosing fibres must be assumed. These anastomoses anatomists have not yet demonstrated. Such a network would correspond to the hypothesis of Marshall Hall, that a special excitomotor system subserved the reflex function. M. Hall has the undoubted merit of being the first (earlier than Johannes Müller, as is acknowledged by the latter himself) who investigated most widely and thoroughly the vaguely known phenomena of movement consequent upon stimulation of peripheral areas of the body; who employed the principle of reflexion from the sensory to the motor nerves through the medium of the central parts of the nervous system; and who, by the discovery of many new facts, indicated the meaning of numerous phenomena in their connexion with other nervous actions, which were, until his time, falsely explained. In his paper in the *Philosophical Transactions*,[1] M. Hall develops the following peculiar view:—He thought that the phenomena of reflexion are limited to the spinal nerves, and do not include the nerves of special sense connected with the brain. Reflexion is not brought about by sensation, not even by sensory nerves, but by special nerves,—the "excitomotory," which conduct the impression, and the "reflectomotory," which cause the reaction. In the mixed nerves these two sorts of fibres are mingled with the sensory and motor nerves. The tonus of the muscles and the closure of the sphincters are maintained by the reflexion of the spinal cord. The reflex movements are as different from the voluntary ones as from those of muscles alone, and form a class by themselves. J. Müller[2] points out that Hall's theory differs from that of Whytt as well as from his own. In the first place, M. Hall limits the phenomena of reflex action to the spinal nerves, and denies to the cerebral nerves of special sense the power of exciting them. He supposes the reflex motor action to be in no case excited by sensation, not even by means of the sensitive nervous fibres. He maintains the existence of special nerves, or nerve fibres, endowed with the excitomotory function; and the reflex action he supposes to be conveyed, not by the nerves of spontaneous motion, but by special fibres, which he calls "reflectomotory." Grainger[3] was a warm supporter of this doctrine. M. Hall did not found his excitomotor nervous system upon the same grounds as led Spies to a similar hypothesis, but because he regarded the psychical and the reflex functions as so very different, that he could not imagine them as occurring in the same way. Beyond this, a special system of fibres seemed to him to be necessary for the reflex processes, for the greatest reflex action does not seem to belong to those parts which are most richly supplied with sensory nerves, and because reflex movements can occur through the spinal cord without consciousness, and after the removal of the brain. Volkmann[1] shows how improbable is the hypothesis of M. Hall, for every piece of skin of the size of a needle-point would require two specifically different fibres—one for sensation, and another for reflex action; and, as Kürschner[2] remarks, just as for a reflex motor action, so also for the discharge of movement by volition or emotion, special fibres are necessary. "It is therefore obvious that, in carrying out Hall's principle, the number of specific fibres in all probability would be so great, that the small nerve fibres would not be able to contain them all."

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[1] Anat. and Phys. Commentaries. London, 1823.

[2] *Loc. cit.*, p. 36.

[3] Article "Nerven-physiolgie," in Wagner's Handwörterbuch der Physiologie, Bd. ii. p. 546.

[4] Lehre von den Reflex Functionen, Arnold. Heidelberg, 1842.

[5] Physiologie d. Nervensyst. Braunschweig, 1844.

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[1] On the Reflex Function of the Medulla Oblongata and Medulla Spinali.

[2] Translated by Baly, vol. i. 768.

[3] Observations on the Structure and Functions of the Spinal Cord. London, 1837.

Pflüger,[3] with the aid of critical dissection of the memoirs already published on this subject, together with cases of disease of the central nervous system, as well as from his own observations, argues strongly against M. Hall's view. He tries to prove that the sensorium is distributed throughout the entire cerebro-spinal organ. Auerbach[4] and Funke[5] agree with him; whilst Schiff[6] says—"Granted that the spinal cord can be affected by stimuli independent of cerebral consciousness, it is still capable of being affected by external stimuli. We must, however, deny to it all that is generally included under the term will or formal conception, and we must carefully guard against the view that the reflex movements are voluntary."

Eckhard[7] inclines to a mechanical view. He asserts that the faculties of the spinal cord are automatic and reflex. He arrives at a similar conclusion to Volkmann, that the known facts do not prove the existence of a psychical function of the spinal cord. What has weighed with him specially in deciding against the psychosis of the spinal cord is, that "individuals suffering from disease of the spinal cord do not mention that they have lost anything except the loss of feeling and of motion."

(*To be continued.*)

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[1] *Loc. cit.*, p. 548.

[2] Translation of M. Hall's Works, Appendix. Marburg, 1840.

[3] Die Sensorischen Functionen d. Rückenmarks d. Wirbebthiere. Berlin, 1853.

[4] Güzburg's Med. Zeitsch. Berlin, 1856. Bd. iv. p. 452.

[5] Lehrbuch d. Physiolog., 1858, Bd. ii. p. 401.

[6] Schiff, Lehrbuch d. Physiologie, I. Lahr, 1859, p. 213.

[7] Experiment. Physiologie d. Nerven-Systems, pp. 252 and 254. Giessen, 1867.

ARTICLE VIII.—*On Rheumatoid Arthritis: Its Diagnosis, Pathology, and Treatment.* By GEORGE W. BALFOUR, M.D., F.R.C.P. Ed., Physician to the Royal Infirmary.

GENTLEMEN,—There are few diseases more deserving of your careful attention than rheumatoid arthritis ; not on account of its fatality, for it does not tend to shorten life ; but because of the suffering it produces and the crippling which results from it ; and also because, while to a certain extent it is amenable to appropriate treatment, that treatment is quite different from that of those allied affections with which it is most apt to be confounded, while, to be of any avail, it must be early applied, and persistently continued till the desired relief is attained.

Rheumatoid arthritis is a disease which may occur at any age ; it is therefore a most important error to regard it as the *malum senile articulorum,* as it has been termed, because, though it may no doubt occur primarily at an advanced age, and more frequently its ravages may first attract serious attention when its victims have begun to age, yet in most instances the commencement of the disease must be carried back to a very much earlier period, and in no small number of cases its more serious development is due to the non-recognition of its early beginnings ; and it is an important fact, that its very worst form has been observed in children of ten or twelve years of age (Garrod), though, as a rule, it may be regarded as mainly a disease of middle and advanced life.

The great evil of the non-recognition of this disease in its early stages is that, when unchecked, it travels from joint to joint over the whole frame, never leaving hold of that first affected. It seizes first one joint and then another till all are crippled and deformed, every movement rendered more or less difficult or impossible, and the victim rendered helpless and crippled for all his future life. Not only his limbs are thus fixed and distorted, but his neck may be stiffened, his jaw locked, and his visage maimed by the deformity of his temporo-maxillary articulation. And yet in itself it is not a disease which tends to shorten life. Its gradual progress, without any intermission, or with but slight remissions, may indeed imbitter all the comfort of the poor patient's life, but it by no means necessarily curtails the length of that life. The first patient whom Dr Haygarth saw labouring under this disease lived to the age of ninety-three ; and we can all recall, I daresay, the crippled figures of many who have spun out a long life in the enforced idleness entailed by this affection. By some this disease is termed chronic rheumatism ; by others, poor gout ; and by many, rheumatic gout, as if it were a hybrid between the two—an opinion which is shared by Dr Adams, of Dublin.[1] Nevertheless, rheumatoid arthritis is certainly no hybrid between rheumatism and gout, but appears to be an essentially distinct disease. Its morbid anatomy and pathology differ *in toto* from that of either of these diseases; and its clinical history, while presenting in general perfectly distinct features of its own, only rarely, in the early stages, simulates at one time acute rheumatism, and at another sub-acute or chronic gout, from both of which, however, it may with care be readily enough differentiated.

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[1] A Treatise on Rheumatic Gout. 2d edition. London, 1873.

In acute rheumatism the constitutional disturbance is great, the temperature high, the skin covered with sour-smelling perspiration, the urine scanty and high-coloured, depositing, on cooling, a copious red sediment; the joints are swollen, painful, and covered with a red blush. The joint affection is erratic, the development of the disease in one joint or set of joints being frequently accompanied by its rapid subsidence in those primarily attacked, and there is a strong tendency to implication of the heart in the form of endo- or pericarditis. Death rarely occurs from rheumatism, and then chiefly from cardiac implication. Perfect recovery is the rule, and crippling of the limbs from acute rheumatism is unknown, though, if the attack has been severe, neuralgic pains continue to affect the joints for some time after. When the joints become crippled from chronic rheumatism, as they sometimes do, it is the tendons connected with them which become contracted, and especially the ligaments, which may become thickened and contracted; but to produce these effects the disease must last for a long time, and they are never associated with the bony exostoses and dislocations which constitute so great a part of the deformity in rheumatoid arthritis.

In acute gout, on the other hand, the constitutional disturbance is never so great as in rheumatism, nor the temperature so high—both being proportionate to the local inflammation, which is not always confined to one joint, though the great toe is frequently one of them. There is much tense and painful swelling of the joint affected, which is of a bright or dusky red, with large veins proceeding from it; as the inflammation subsides, the swelling pits and subsequently desquamates. The urine is scanty and high-coloured, depositing a pink or brick-red sediment. Perspiration is either absent or but trifling in quantity. There is no tendency to acute cardiac implication, though in advanced life gouty dilatation of the heart constitutes a most serious complication. After recovery the joint may remain œdematous, or stiffened, or it may be even anchylosed; but it is only after long-continued attacks of chronic or sub-acute gout that they become so deformed as in some degree to simulate the results of rheumatoid arthritis; still, to an experienced eye, there is no difficulty in distinguishing between the two. In gout the deformity is not caused by bony deposit or dislocation, but by the joint being embedded in urate of soda, as bricks in mortar, irregular masses of this being deposited round the joint, infiltrated into every bursa, plastered along the shafts of the bones, or in the tendinous sheaths of the muscles. I have seen several cases of greatly deformed joints of this kind, but have never seen one which could not be readily recognised to be gouty and not rheumatoid in character from the appearance alone. Frequently some part of these irregular swellings is soft and pultaceous, and occasionally the chalk stones, as these deposits are termed, when hard and dry, project through the skin, when their true character may be readily determined. In cases where the symptoms are more obscure, tophaceous deposits may be sought for in those other parts which gout marks peculiarly as its own—such as the lobes of the ear, tips of the fingers, or the bursæ over the olecranon; or in more doubtful cases still, the uric acid may be sought for in the serum of the blood; its presence, when found, conclusively proving the gouty nature of the case. In many cases it suffices to apply a small blister, acidulate the serum thus procured with a small quantity of acetic acid, and place in it an ultimate fibre of linen cloth, round which the uric acid crystallizes. The detection of uric acid in this way may be regarded as quite satisfactory; but a negative result is not so, and must be checked by the detraction of a small quantity of blood from the patient, the serum of which must be treated in the same manner; if the result still be negative, we may be satisfied that we have not to do with gout. Perhaps the most troublesome cases of this doubtful character are those in which, with a history of hereditary gout, and the presence of a gouty kidney, evinced by slightly albuminous urine, but without any preceding attack, though probably well deserved, or any actual gout at the moment, we have at a somewhat advanced age the occurrence of pains in thigh and hip-joint as the result of the development of rheumatoid arthritis as the *malum senile coxæ*, no other joint being affected. In one well-marked case of this kind, the patient, with a hereditary tendency to gout, and well-marked gouty kidneys, during the course of a long life had, according to his own confession, drunk both port and claret sufficient to float a man-of-war, yet even in him the situation of the pain and the slight flattening of the affected hip were sufficient to show that the pain was due to the altered relations of the parts when motion was attempted, and not to any gouty inflammation; and the subsequent development of the case in the course of years completely confirmed the diagnosis.

Rheumatoid arthritis is never fatal of itself, and never gives rise to any fatal implication of internal organs; it may be associated with disease of the heart, liver, or kidneys, arising from other causes, but it never originates them itself. It is the chronic rheumatism, so called, which deforms—hence one of its names, arthritis deformans; and it deforms more often than it ought for want of proper treatment. It does not come very often before us in the Infirmary, yet no session passes in which we do not see several cases either as in or out patients; these are about equally divided between the sexes. Haygarth indeed says, that having observed the disease for twenty-six years, he had only once seen it in a man; modern experience is, however, more consonant with our own, in that sex is not believed to exercise any influence in producing it. It may also occur at all ages, but it does not occur with equal frequency in all places; and such damp localities as Ireland and Holland have an undoubted pre-eminence in this respect. It may occur in an acute form, which may be primary; possibly the disease may always begin thus, being too frequently not recognised; at other times, an acute attack supervenes upon an already well-marked disease. The constitutional disturbance is never great; the temperature above the normal, but not greatly so; perhaps 102° may be about its upper limit; there is no excessive perspiration, the skin being usually natural; the urine may vary somewhat in each case in colour and quantity, but is not abnormal; the joints are painful and swollen, the swelling being limited to the synovial capsule; the joint affection is not erratic, and may be limited to one or two, or extend over many; there is no tendency to heart affection. In this form the disease, if unrecognised, is most obstinate; it resists all ordinary anti-rheumatic or anti-podagric treatment—is, in fact, rather worsened by such treatment; by-and-by, however, under the influence of warmth and rest, the pain gradually ceases, and the patient is discharged improved, with joints still somewhat puffy. When the patient next comes under observation, it will be learned that the remission has been but of short duration, the aching pain has soon returned, and with occasional worsenings has persisted, and that now the ends of the metacarpal bones are enlarged, the fingers incline towards the outer or ulnar side of the hand, all the affected joints are enlarged and deformed by hard bony swellings exterior to them, are either immobile, or, if passive motion be still possible, it is accompanied by pain and by a crackling sensation, and ultimately there is developed an alteration in the ends of the bones, which, especially in the ball-and-socket joints, simulates fracture, and ends in displacement and dislocation. In the early stage of this disease the only change discoverable is simply effusion of synovia with vascular injection, the signs of arthritic inflammation; at a later stage the fluid becomes absorbed, and the cartilage ulcerated. At a very early period the cartilage splits up and becomes removed, apparently by a slow process of absorption. It has been alleged, indeed, that the essence of this disease consists in irritative proliferation of the cells of the cartilage, producing a softened and overgrown condition of its substance; that this softened cartilage is unable to withstand the mechanical forces which act upon the skeleton, and consequently gets worn down and destroyed at those parts subjected to the most active friction during the movements of the joint, the overgrowth maintaining its ground only at the edges of the cartilage; and this bulging out round the joint ultimately becomes calcified, giving rise to those bony exostoses so constantly found surrounding joints thus affected.[1] But though this idea is a plausible one, it is of doubtful verity, not only because erosion of the cartilages, which ought always to lead to similar results, is greatly more common than rheumatoid arthritis, but chiefly because the further development of the theory leads to views inconsistent with the actually ascertained morbid anatomy of the disease. As the disease progresses and the cartilage is removed, the denuded ends of the bones come into contact with each other, and are found to be polished and eburnated in a remarkable manner, the articular surfaces being covered entirely or in patches with a porcellaneous deposit, which is frequently streaked or grooved in the direction of the motion of the joint. This is alleged to be due to the mechanical condensation of the old bone;[2] and Rindfleish asserts that, as the brittle bone-tissue is as incapable of resisting the continued rubbing as two pieces of pumicestone, it is gradually worn away, and thus the most extreme distortion of the joint becomes possible.[3] The same author concludes that this process of eburnation is brought about by what he calls a tendency of the bone "to protect its medullary spaces, towards the articular cavity, by an ossifying ostitis." But among horses, who are extremely liable to rheumatoid arthritis, we often find this process of eburnation well marked in positions where there is no tendency to produce this mechanical rubbing-down of the ends of the bones, and therefore no need to protect its medullary spaces; as, for instance, in the os naviculare —a sesamoid bone, not exposed to friction or pressure at all—in which, nevertheless, this eburnation is not unfrequently found throughout its whole texture, and not merely confined to its surface, as if this process was an integral part of the disease, and did not depend upon friction at all. Even in man we occasionally find the head of the femur or of the humerus completely eburnated, and, as it were, changed into a semblance of an ivory billiard-ball, without any trace of porcellaneous deposit or frictional striæ being found upon the surface. But it is especially in the horse, whose morbid anatomy in relation to this disease is singularly rich and instructive, that we most readily observe all the stages of this peculiar form of ostitis. For in them we find that, while the articular cartilages are undergoing their peculiar degenerative softening, the subjacent bone is gradually becoming hard and eburnated; and in the sesamoid bones this change takes place without any observable alteration of the cartilage covering them. This change is effected by the formation of a very imperfect bone, a sort of calcification of the already existing bone-tissue, in which no bone-corpuscles are to be detected, and this calcification bulges out around the joints in the form of irregular nodules, displaying a similar structure. Nay, it is even found within the joints, which are often full of foreign bodies of various sizes, sometimes cartilaginous, at others calcified, with a hard exterior and a soft interior, or the reverse, sometimes free within the joint; at other times, and most commonly, attached to one another, or to the interior of the joint, by a ligamentous pedicle. The calcified nodules exterior to the joint frequently interrupt the movements of the articulation, and produce a form of false anchylosis. By-and-by, this eburnated or degenerated osseous tissue takes on a destructive perforating ulceration, which gives a peculiar worm-eaten appearance to the diseased bones, and it is then that those alterations of form are most apt to take place which have been referred to by Rindfleisch to mechanical agencies, which they no doubt partly are; but, in the horse especially, these worm-eaten bones are often found unassociated with any well-marked change of form. It occasionally happens that during the progress of this perforating ulceration of the bony tissue, a more healthy inflammatory action is set up, and we have at this stage a true anchylosis brought about; but this is a rare occurrence. More usually the disease now progresses to those subluxations and deformities which are its most common termination. It is a curious and suggestive fact, that Mr Tomes has described a change corresponding to the eburnation of bone as occurring in that part of a tooth lying beneath a carious cavity; and Mr Paget has stated that, so far from this change being protective in either situation, the destruction appears to be most active where there is most induration.[1] All these facts rather tending to show that rheumatoid arthritis is primarily and mainly a disease of the bones rather than of the cartilages; and though the articular ends are principally affected, the disease is not confined to them, as is shown by the affection of the sesamoid bones referred to.

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[1] *Vide* Rindfleisch, Pathological Histology, New Syd. Soc. Trans., vol. v. p. 267.

[2] Wilks, Pathological Anatomy, 2d edition, London, 1875, p. 76.

[3] Op. cit., p. 268.

The etiology of this disease is sufficiently obscure. When we find it so common in an animal so abstemious as the horse, we may readily and safely acquit all alimentary excesses, all over-indulgence in beer, wine, spirituous liquors, or animal food from any share in its production. But we are not so safe to exonerate the stomach from all share in inducing this disease, because the horse has a stomach as delicate as any lady's, and the irregularity in its meals, to which it is so frequently exposed, may induce some form of dyspepsia, which may only reveal itself in this fashion. For this, however, there is no proof. It is more to the purpose to say that the blood, the urine, and the perspiration have all been analyzed, with merely negative results; the obvious inference being, that it is more probably a local than a diathetic disease.

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[1] Surgical Pathology, 3d edition, London, 1870, p. 302.

In the horse, excessive work might well be regarded as a probable cause; but in man this is by no means always the case, for it is often found in females who have done no hard work at all, and it is by no means more frequent in males, who are much more exposed to the deteriorating influences arising from that cause. It is not, however, uncommon in families of a phthisical tendency, and it is specially apt to occur in those who are of an enfeebled habit of body, either congenital or acquired. Cold and damp have been recognised as two of the most important factors in inducing this disease, and these are perhaps the only two which are undeniably common both to man and the horse; while it is so far corroborative of this view, that Ireland and Holland, both possessing cold and damp climates, are countries in which this disease has been noted to be specially common. Cessation of the menses is occasionally accompanied by an improvement in the condition of those affected with this disease; but, on the other hand, cessation of the menses is not always a favourable circumstance in relation to this disease, for in many instances it first makes its appearance at this period. In short, the only factors in the production of this disease, as yet recognised as effective, are exposure to cold and damp and debility of constitution, congenital or acquired; but these are only assistant factors, or, as we term them, predisposing causes, and the actual ultimate cause has as yet eluded all attempts to discover it.

The prognosis in this affection depends very much upon the stage in which we find it. When the joints are already stiffened and deformed by calcareous nodules, or rendered abnormally mobile, or already dislocated by the combined effects of internal absorption and mechanical pressure, nothing short of reconstruction of the frame could restore them to their pristine state. But when the patient is seen early enough and properly treated, there is every reason to expect that a more or less perfect recovery may result. Early recognition and accurate diagnosis exercise a most important influence upon the prognosis in this disease.

Knowing the difficulties which beset our attempts to treat rationally any disease, even when it has a definite pathology, you may well ask, upon what principles we can hopefully expect to treat a disease such as this, so rich in its morbid anatomy, so obscure in its pathology? But to this it may be very well replied, that even when the pathology of a disease is best known, our most successful treatment is too often only a successful empiricism, whose mode of action has received a varying explanation in each succeeding age; and it is only in the rarest instances that we can effectively demonstrate a rational connexion between the administration of a remedy and its supposed curative action. Still we know, that in accordance with the doctrine of probabilities, any case, however improbable, if it be a recurrent one, is as much entitled, under the laws of induction, to a fair valuation, as if it had been more probable beforehand (La Place). Hence, if the use of any one remedy continues to be followed in successive ages, and in the hands of successive practitioners, by the alleviation or cure of the disease for which it is employed, we are bound to accept that remedy as a distinctly curative agent, even though we cannot explain its action, unless indeed we can prove that its use has been superfluous ; that it has been more successful than other remedies, simply because less perturbative, and that nature and not art has been the important agent in promoting the cure. In regard to rheumatoid arthritis, we know that, as a rule, of which past experience has supplied only too many examples, left to nature, it progresses unchecked from joint to joint till the unfortunate sufferer is totally crippled and dreadfully deformed. To this rule, however, there have been many exceptions ; and when we examine those exceptions, we shall find that they have all been due to improvement in the health of the patient, from some definite cause, such as cessation of childbearing in women ; or to an improved position in life attained by the patients, which has enabled them to avoid exposure and fatigue, and to feed better than formerly. Under these circumstances this disease is often checked, and ceases to progress beyond the stage to which it had attained when the vital condition of the patient became altered. We learn from this, as well as from all that we know of the pathology of this disease, that the most important part of the treatment must consist in making the patient as comfortable as possible, causing him to avoid all exposure and fatigue, and removing as far as possible all sources of debility ; at the same time that we feed him as well as we can, and stimulate him when necessary by a moderate allowance of alcoholic drinks. The great bugbear in regard to the adoption of this tonic regimen has been the epithet rheumatic *gout*, so long applied to this disease, the mere name having too often influenced the treatment. Hence we find Sir Benjamin Brodie recommending placing the patient upon a carefully-regulated diet, causing him to partake but moderately of animal food, to avoid fruits, acids, raw vegetables, and sugar ; to take little or no spirituous or fermented liquors ; and to take daily exercise sufficient to further gentle perspiration. But alas for all these fanciful regulations ! in the horse we have an animal who indulges in none of those forbidden articles—except perhaps the raw vegetables, and we do not know that even Sir Benjamin would include oats and hay under that term—and who daily perspires at his work, yet no animal is more subject than he to this disease. Be not therefore afraid to feed your patient well, if by so doing you can improve his health ; because in that improvement lies your great hope of cure, and only a diet which impoverishes can rightly be regarded as injurious. When we come to more active medication, we find one uniform testimony that all depressants, including alkalies and colchicum, so useful in rheumatism and gout, are positively injurious in rheumatoid arthritis. In it all the treatment must be strictly tonic to be beneficial, though some of these tonics may be selected for special purposes—thus, cod-liver oil is useful in many cases when there is wasting, and chalybeates when anæmia predominates. But of all the tonics that have been found most useful, from the days of Haygarth,[1] down through Noel Geneau de Mussy to Fuller and Garrod, none has surpassed arsenic, which may be combined with other remedies, but in itself is, I believe, the only tonic to be certainly trusted in as a curative agent; and I quite agree with Fuller, that "no case of rheumatic gout can be regarded as incurable in which its influence has not been fairly tried." I have never known it to fail in checking the disease, if faithfully persevered in, but it must often be taken for many weeks, and always in full doses—such as 10 or 15 minims of Fowler's solution three times a day. Along with the arsenic other needful remedies may be conjoined, if necessary; for the cure of the disease, or, at all events, for its arrest, for a perfect cure is simply impossible, the arsenic seems quite sufficient. Trousseau and Lasègue also strongly recommend iodine in the form of tincture in doses of from 10 to 90 minims three times a day, along with food; but this remedy has not been successful in other hands, and the arsenic has so long maintained its reputation that it is unnecessary to experiment with more doubtful medicaments. When there is much effusion into the joints, or in the early stages even when there is but little effusion, I am frequently in the habit of conjoining the arsenic with iodide of potassium with apparent benefit. But you must not forget that Lasègue expressly states that the iodine has in this disease a distinctly different action from the iodide of potassium. There is, however, one manner of using iodine which you will certainly find of the greatest benefit, and that is the application of iodine paint over the joints affected, conjoined with the internal use of the arsenic; in this way the pain even of an acute affection is speedily relieved, and the disease ultimately arrested. A warm climate, warm baths and douches, especially those of natural thermal waters, are useful in such cases; hot saline douches are also beneficial; whatever stimulates the cutaneous circulation seeming to do good in such cases; and in this way guaiacum, especially as the ammoniated tincture, may now and then be beneficial; but you may be certain that nothing will so surely relieve your patient and check his disease as the internal use of arsenic, with the external use of iodine, and if much pain be present and warmth be grateful, the affected limb may be put in a box and covered with hot sand at a temperature of about 140° F., which does not need to be renewed often, as the sand parts but slowly with its heat. Faradization and galvanism have both been recommended for the relief of pain in this complaint, but will be found less frequently applicable, or indeed required, than in many cases of true rheumatism of a chronic character. All those mineral waters which act powerfully on the skin, bowels, or kidneys, are injurious in this complaint, but chalybeate mineral waters may be useful in improving the health of the patient, and in thus promoting a cure.

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[1] In Christison's Dispensatory, 1842, p. 186, Haygarth is credited with having first employed arsenic in nodosity of the joints. In Haygarth's own tractate on the subject (Bath, 1805) there is, however, no mention of arsenic. As yet, I have been unable distinctly to trace the specific employment of arsenic in this disease beyond Fuller; at present, however, I leave the statement as it stands.

In the course of the treatment it is often very remarkable how much of the articular rigidity disappears with the acuteness of the symptoms which have led the patient to seek relief; but there is a limit to this, and that once attained, no further improvement can be effected. The abnormal mobility, the result of luxation of various degrees, is not susceptible of any relief, except by mechanical appliances, and that only rarely. When any of the exostoses are inconvenient from their size or their situation, as when they form in the sole of the foot, which occasionally happens, they may be removed without more than the ordinary risk arising from such operative interference.

NURSING IN THE EDINBURGH INFIRMARY.

Of late years, especially since the Crimean War, the whole subject of nursing in families, hospitals, and camps has been prominently before the public mind. The "Lady with the Lamp" not only helped to save the lives of our soldiers, but, by her influence, pen, and example, has done much to put the whole question of nurses and nursing on another footing.

The novelist of to-day, were he describing a sickroom and its occupants, would no longer be justified, or even tempted, to portray a Mrs Gamp. The picture now is very different; and even a satirist would describe, not a coarse old beldame, but an active, clever, at the worst conceited and lively young woman, as the typical hospital nurse.

Judging from correspondence in public prints and conversation in social circles, the time has now arrived for estimating what we have gained by the change, and indeed what this change really is.

In what does this great change consist? First, the very simple discovery was made, that nursing did not come to all women by intuition, that it was a difficult art, requiring not only temper, firmness, obedience, and that great faculty capableness, but also that a certain amount of training and instruction rendered all these good qualities ten times more valuable; and, secondly, such being the case, it gradually dawned upon the minds of hospital managers and doctors, that such women required to be sought out. The old method of selecting hospital nurses seemed to be, to accept at small wages all the domestic servants or widows, who from various reasons did not get on in ordinary domestic service,—the clever ones amongst them either had temper, or had no character; the stupid ones were probably idle or shiftless.

Some of these, no doubt, found their vocation, and, by a sort of natural turn for it, became valuable nurses, their value often having temporary eclipses during a drinking bout, and being lessened by coarseness of voice and speech. Others had their good qualities too, but each with a fatal *but;* good-tempered, but lazy; kind and motherly, but a hopeless slattern: few could write a letter, hardly one could give an intelligent answer to a question.

Exceptions there were, whose supreme excellence and capacity cannot be overestimated, and for whose sake it is difficult for us older men to hear a word against the old system. But if the rank-and-file of even the day-nurses were inefficient, who shall tell the delinquencies of the poor old charwomen and scrubbers, who were by courtesy called night-nurses? Worn-out, to begin with, underfed, underpaid, allowed only five or six hours' rest after a day of scrubbing, before beginning a night of watching, what wonder was it that the watching was a farce; and that the poor old drudge drowned her cares in whisky or laudanum, and rested her weary back in any unoccupied bed in the ward? When the writer was a house-surgeon (*consule Planco*), one of the duties of a really active and interested house-surgeon was to go round the wards about the mid-watch and rouse the night-nurses. Hence for really urgent and severe surgical cases, the dressers and house-surgeons had to take the night-nursing in their own hands.

And over this unorganized crowd, without *esprit de corps*, without traditions, the only superintendence was that of an already overworked housekeeper, for whom the kitchen, the laundry, and the storeroom were a heavy enough burden. The wonder is, things were no worse.

When lads of twenty-one had to dismiss and appoint night-nurses, with none to select from except washerwomen who had not succeeded, or night-nurses who had probably been dismissed from another ward the day before; what wonder that they did not have much satisfaction in their selection? And when the surgeon, if driven by desperation to dismiss a day-nurse, had only the least bad of the night-nurses to choose from, no wonder we were afraid to meddle, and had to wink at much evil and neglect.

The difficulty which met those who wished to improve the whole tone and style of nursing was the want of proper material to make nurses of. Clean, capable, steady, strong women with characters, knew their own value, and easily found employment. The only way to obtain such was to improve the position. This has been done by the most obvious method of improving the conditions of the nurse proper, by limiting her work. On the old system, the day-nurse was cook and housemaid as well as nurse ; the night one was drudge, scrubber, and only incidentally had to take the most difficult and trying post of nursing twenty sick and dying men during the long hours between eleven and seven. Hence the division of labour between nurses, night and day, and ward-maids or scrubbers.

Again, a trained nurse after a year or two is worth a great deal more than a fresh hand; hence the division into staff nurses and probationers who in their turn will become nurses.

The change had to be a gradual one; the old day-nurses had to be gently dealt with—some, with a humility and simple loyalty deserving all praise, joined the new system, and are doing excellent and devoted work. Some were pensioned off. Some left, finding that times were changed, and they could not change with them. The old night-nurses, under careful watching and an improved status, made good scrubbers and ward-maids ; and by degrees, thanks to the zeal and energy of those ladies and nurses who began the work, vacancies have been filled up, and a fairly numerous staff of probationers of excellent quality are being gradually trained to be ready to supply blanks made by promotions or resignations.

Already the good effects of the change are seen in the zeal and self-devotion of many, the high character of all the staff nurses. A mild but firm discipline is possible when a nurse knows that her place can be supplied in a moment. The night-nurses are of the same stuff as the day ones. All are systematically and carefully trained to be capable, useful aids to the physician or surgeon.

The system of training is very complete. Probationers, on joining the staff, are placed under the head-nurses in charge of wards, by whom they are first inducted into the more mechanical part of the work ; for though they have no menial duties to perform, it is thought well that they should know all that has to be done in a ward, and how to do it. They are then taught the essential and practical duties of a nurse in the care of patients, at the same time being instructed by means of classes held by the ladies superintendent as to the why and the wherefore of all they do and see done. Having acquired a certain amount of knowledge both practical and theoretical, they are permitted to attend lectures of a more advanced order, which are most kindly given by many of the medical officers of the Royal Infirmary, on subjects strictly allied to the profession they have adopted. These lectures are given by Mr Joseph Bell on Surgical Nursing and Surgical Appliances ; by Mr Chiene, on Anatomy ; by Dr Croom, on Obstetrics and the Diseases peculiar to Women ; and during the present season, on Physiology, at a special class for ladies, by Dr M'Kendrick. The character of the instruction being particularly designed to make nurses, not doctors,—nurses who shall not only know what is best to be done for a patient under all circumstances, but shall also be of valuable assistance to the doctors, being able to give an intelligent and correct history of the intervals, often long, which necessarily occur between professional visits. These lectures are attended most assiduously, and are thoroughly appreciated by the probationers and nurses; and it is most gratifying to observe the kindly interest with which the lecturers regard their pupils—to the most efficient of whom prizes are given as the season closes. Such a system of training cannot but produce good nurses, to the comfort and wellbeing of patients in the Infirmary, and it is hoped ere long, when the establishment is fully developed, of those of the wealthier classes, whom sickness may compel to apply for help to the Lady Superintendent of Nurses in the Royal Infirmary.

They can give an intelligent account of the state of a patient, and can be trusted to watch symptoms and report at once. Above all, there is an *esprit de corps*, loyalty, and self-respect, which has a most salutary influence on the whole tone of ward life—on patients—even on students.

The coarseness, levity, and hardness, which used unconsciously to leaven the manners and atmosphere of the wards, has in most been greatly modified and softened.

The solitary meals snatched at odd times by the old nurses in their own rooms are replaced by a comfortable meal in a common hall, the cheerfulness and society of which must be a great rest.

We need not speak here of the different classes of society from which hospital nurses are now drawn—that many ladies by birth and breeding are now staff nurses is not a necessary part of the scheme; but it is a great advantage in many ways, and we fearlessly assert that, under the present system, a lady whose inclination leads her to it, can be a probationer and nurse in a great hospital without the loss of one iota of her self-respect, without danger to her health, and can lead thus a happy, useful, and honourable life. For, to be successful in her work and earnest in her training, the nurse should feel that she is taking it up as a profession, not as a mere freak or pastime; a profession most useful and honourable—useful, for, as Miss Nightingale long ago showed, many rich lives as well as poor ones are lost for the want of good nursing—honourable, as it may be the means of advancing medical science in many ways. The constant watching of the sick, and consequent close and accurate observation of a trained nurse, may do much for science. As Mr Hinton says, "With a class of trained women ever at the bedside, skilled in observing with the utmost accuracy, and without disturbance to the patient, all those delicate variations disease presents, medical knowledge itself might be expected to enter upon a new development; new subjects and methods of observation could hardly fail to develop themselves."

It has been objected that the new system is expensive: it does cost money—everything, from surgical dressings to horseflesh, is dearer nowadays than it used to be: but if it be remembered that a highly-trained staff-nurse, with charge of perhaps thirty patients, gets at the highest figure £25 a year, while the probationer gets £10 per annum, they can hardly be said to be overpaid.

But were the expense double what it is, no one who knows the difference the last twenty years has seen in our great hospitals as to this matter, would grudge a shilling that has been spent in raising the status and character and career of that most valuable class—hospital nurses.

Part Second.

REVIEWS.

Lectures on Surgery. By JAMES SPENCE, Professor of Surgery in the University of Edinburgh. New Edition. Edinburgh: Adam and Charles Black: 1876.

ALL Edinburgh men—in fact, all men interested in the practice of surgery and in its progress—will welcome a second edition of Mr Spence's Lectures. The necessity for its appearance is matter for congratulation, alike to the author and to the profession: to the author, as the direct acknowledgment that his labour has been recognised and appreciated; and to the profession, that it gives it an improved reprint of a thoroughly good book. Turning to its pages we find much that is new and valuable in them, the value and the instructive power increased not a little by the addition of many well-executed woodcuts.

The structural classification of Tumours has been carefully and well rewritten, and we note with interest a case of recurrent tumour, the whole bearing of which seems to us to support the views of Creighton, who, in the clinical history more than in the histological appearances, finds the true definition of malignant and benign.

In the chapter upon Diseases of Bone, acute necrosis and its treatment by subperiosteal resection is fully and ably considered, and the value of the procedure pointed out.

The consideration of Fractures, their varieties, their mode of union, and their treatment, gives opportunity for the introduction of much that is fresh,—the wide application of the extension method by pulley and weight, the safer use of the hooks of Malgaigne, and the best and safest treatment of ununited and badly united fractures. The lectures upon hæmostatics now embrace a description of the bloodless method of Esmarch—of the value and uses of the galvano-cautery and ecraseur—and of the reintroduction of catgut ligatures; and here let us say, not without want of due deference, that we think the value of this last application is, by the author, much underrated. As the days pass, and the antiseptic system of surgery finds a wider field, its latest and possibly its greatest achievements have been accomplished in connexion with this use of catgut.

The second volume, that upon Practical Surgery, opens aptly with a most instructive lecture on the treatment of patients before, during, and after operation. Anæsthetics, their mode of use, and their respective value, are discussed; and, with what might be thought true Edinburgh feeling, chloroform is allowed to bear the palm.

The increased statistics of amputation and excision are, we think, of more value than those previously published, inasmuch as they teach many lessons as to the varied conditions and complications unfavourable to the success of operations. Mr Spence's experience of primary amputation has been a most exceptional one, and the truths taught by it are well told; one strikes us forcibly—it is this:—"As a general rule the less of the limb removed the less is the risk, but there are exceptions to all rules; and in cases where a bone is much shattered, the risks of acute necrosis and osteo-myelitis attacking the bone of the stump, and so inducing pyæmia, are so great as to warrant in many cases amputation at or about the neighbouring joints." How thoroughly this holds good, especially in primary amputation for compound fracture in the upper third of the leg, most surgeons will realize. As to the identity of croup and diphtheria there has of late been much difference of opinion. That they are essentially different diseases, Mr Spence is very positive; and the value attaching to this view, from his great experience in cases of tracheotomy, cannot well be over-estimated. As regards the operation and the necessity for it, with him the special disease makes no difference, his prognosis and after-treatment it however influences much.

When we first heard and then read the lectures upon Head Injuries, Hernia, and the Diseases of the Urinary Organs, it seemed to us impossible that they could with any advantage be added to, but even here there is something new.

Increased experience has matured previous opinions—it has taught fresh lessons, nothing of which has been lost. To almost all the practical divisions new clinical cases have been added—many of them certainly of much rarity—all of them interesting and instructive, and all, in their consideration and result, tending to add somewhat to the wide and well-deserved reputation of the man whose name is associated with them.

Truly he has laboured in a broad and rich field, and with such a field and such a labourer the fruit was sure to be good. How good it is they best know who, practising surgery, meet with many difficulties, and who, referring to this book, will aye find the surest and readiest way out of them.

To the student it must be an easy and pleasant text-book; to the practitioner, a most valuable book of reference.

Catalogue of the Models of Diseases of the Skin in the Museum of Guy's Hospital. By C. HILTON FAGGE, M.D., Curator of the Museum, etc. London: J. and A. Churchill: 1876: Pp. 270.

SUCH is the modest title of a work which must have cost its author an immense amount of labour; and which, while it facilitates the study of the magnificent collection of models of skin diseases in Guy's Hospital, is also in itself a most important contribution to the scientific nomenclature and arrangement of skin diseases. The descriptions are most careful and vivid; the little histories of the cases, which are attached where it is possible, are interesting; and the whole has had the additional advantage of being corrected by the exceedingly ingenious and laborious artist, Mr Joseph Towne, who for forty years has laboured at the models both of skin disease and of anatomy which adorn the museum. Dr Hilton Fagge has introduced a method of classifying skin diseases, which he thus describes:—"Instead of aiming at a strict classification of cutaneous eruptions, we may content ourselves with dividing them into what appear to be natural groups:—1. Some affections are caused by the presence of parasites. 2. Others affect mainly the appendages of the skin, the hair, glands, or nails. 3. Others are characterized by their destructive tendencies; others again comparatively seldom produce destructive changes in the skin; and of these some (4) are not inflammatory, while others (5) are more or less decidedly inflammatory in their nature.

De la Tête du Fœtus au Point de Vue de l'Obstetrique. Par le Dr P. BUDIN, ancien Interne, etc., etc. Paris: Doin: 1876.

On the Head of the Fœtus in an Obstetrical Point of View. By Dr BUDIN, formerly a Resident of the Hospitals, etc., etc. Paris: Doin: 1876.

WE commend to obstetricians this excellent monograph. It contains much of the best kind of work, that must be studied by all who are abreast of the science of the day; and it gives promise of more of a like kind.

No better evidence can be given of the progressing condition of any department of medical science than its entering on an experimental stage, on the kind of work to be carried on in the obstetrical laboratory, which Dr M. Duncan has said to be as much required as the chemical or physical. Not a little of this work has been recently carried on in Paris, and it indicates a revival there to which Dr Budin has the honour of materially contributing.

Those who think the question of version and podalic extraction *versus* forceps is a settled one; those who consider delivery by the feet easier in all cases than by the forceps; those who think more power can be used by the feet than by the forceps, will find material here to be carefully remarked and inwardly digested.

Budin gives, on the whole, an excellent account of what he professes to describe. But he appears to have omitted a part of the process of the shearing of the fœtal head, inasmuch as he has not described the changes in the shape of the bones of the head that occur in that alteration.

Evolution of the Human Race from Apes, and of Apes from Lower Animals: a Doctrine Unsanctioned by Science. By THOMAS WHARTON JONES, F.R.S., F.R.C.S., etc. London: Smith, Elder, and Co.: 1876.

SUCH is the rather pretentious title of a little work, the avowed aim of which is to overthrow evolution in general, and in its more particular aspects as well. The author gives us both an "advertisement" and "preface,"—the reason for this double introduction not being clear. He says in the former, that the pages of his book "comprise two lectures originally delivered to audiences of ladies and gentlemen in the Botanical Theatre of University College, London," and that he now publishes them "in the hope that they may serve as a means of counteracting efforts, made throughout the country by half-informed persons, to propagate the doctrine of evolution." Further on, Mr Jones somewhat nullifies the effect of his own epithet of "half-informed," by talking of the "more logical of the propagandists referred to;" and we opine our author's statement about the "half-informed" state of some persons as to evolutionary doctrines, may be illustrated from his own pages and by his own case. We have read Mr Jones's sixty-nine pages of large print through, without being able to discover anything which can tend to weaken or otherwise affect the doctrine of evolution. The second lecture is merely a condensed *résumé* of Hæckel's recent popular works. Of one thing Mr Jones may fully rest assured, that unbelief in the existence of a Creator, is *not* necessarily "the alternative of the admission of evolution;" and if evolutionists are to be charged with sometimes being somewhat irrational and illogical, they may point to such works as Mr Jones's, as an argument of *tu quoque* kind.

At any rate, we have found nothing in Mr Jones's pages to enable us to agree with his concluding imprimatur in page 68, in which he says, "We thus see that EVOLUTION, from beginning to end, is an *unverified* and *unverifiable hypothesis.*" The same thing might equally truly be said of many of the beliefs to which Mr Jones pins his faith, or which we presume were taught by Mr Jones's "ancestral kinsman" (mentioned in page xviii.) "brave old Bishop Bedell, of Kilmore."

The Fungus Disease of India: A Report of Observations. By T. R. LEWIS, M.B., and D. D. CUNNINGHAM, M.B. Calcutta: Office of the Superintendent of Government Printing: 1875.

MESSRS Lewis and Cunningham have already made themselves widely known by their respective researches on Hæmatozoa, and atmospheric germs: and a subject like the present, dealing with the common territory on which biologists and pathologists may meet, was naturally one which, to our authors, presented much that was congenial to their investigative tastes. The fungus disease of India, rare in Calcutta, is nevertheless a lesion of grave importance in other parts of India, and was first described by Dr Godfrey of Madras, under the name of "tubercular disease of the foot." The disease has been known for over thirty years, and the belief in its fungoid nature and origin has, in consequence of the researches of Dr Carter of Bombay, and those of the Rev. Mr Berkeley the fungologist, been very generally accepted: the name "mycetoma," given to the lesion by Dr Carter, finding a place in the "Nomenclature of Diseases" of the London College of Physicians.

The authors of the present work have devoted several years to the study of the lesion, and a perusal of their work has convinced us, that whatever exception may be taken to their conclusions, their experimental work has been of the most laborious and careful kind. Without going into details, we may state that the conclusion at which the authors have arrived regarding the nature of the fungus disease, is that its *cause* has not been found to be of fungoid nature, although fungoid growths may appear in the course of its progress amongst the diseased products. The fungi, in fact, are believed to have no more intimate connexion with the disease, than that of attendant plant-organisms, which grow in the soil afforded them by the primary diseased action. But, admittedly, our authors give no explanation of the production of fungoid growths in deeply-imbedded tissues, and in situations far removed from the exterior. It will be for Dr Carter and Mr Berkeley to narrowly examine the work and labours of the authors, who declare their strong belief that the fungus *Chionyphe Carteri* of Carter and Berkeley does "not cause the disease," and that "it cannot be developed from the fungoid elements contained in the morbid product." The question is thus reduced merely to one of cause and effect, and it now rests with Dr Carter and Mr Berkeley either to further strengthen, or, on the other hand, to resign their position in maintaining that the fungoid growth just named is the cause of, and not merely a subsequent and purely accidental occurrence attendant upon the graver and primary pathological action. The *brochure* is illustrated by two coloured plates and eleven woodcuts.

The Soil in its relation to Disease: A Report of Observations. By T. R. LEWIS, M.B., and D. D. CUNNINGHAM, M.B., Special Assistants to the Sanitary Commissioner with the Government of India.

As indicated in their opening remarks, "the present Report embodies the results of observations" by Drs Lewis and Cunningham, "which have been carried out with a view of determining to what extent peculiar conditions or changes of condition in the soil in Calcutta affect the prevalence of disease in general, and of certain diseases in particular." These researches, although possessing a local bearing, may yet form good models for our home sanitarians to imitate and copy. Our authors wisely remark that "it is only on prolonged and continuous observations in various localities that definite conclusions can be based;" and, judging the present production by their own standard, we need say little further than to commend the general manner in which their various experiments and observations were conducted. One of the most important points which the present observations seem to assert is, that cholera in Calcutta appears at its minimum, when the water-level is at its maximum, and *vice versa* (p. 15). Fevers prevailed most when a maximum of atmospheric carbonic acid and the highest water-level were attained. Dysentery attained its maximum once at a point in the rise of the water-level, and similarly at a corresponding point in the course of the fall of the water-level. In Calcutta, it is to be noted, the water-level is dependent on the local rainfall. The Report is very fully illustrated by copious tables, and charts of variations of the temperature of the soil, variations in fever, etc.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LV.—MEETING V.

Wednesday, 1st March.—DR LITTLEJOHN, *and afterwards* DR MATTHEWS DUNCAN, *in the Chair.*

I. *Dr Cadell* read a paper on a CASE OF LYMPHATIC INFLAMMATION, SIMULATING VENEREAL DISEASE, which will appear in a future number.

Mr Bell thought the members would not like Dr Cadell's interesting paper to pass without remark. What struck him as the great point in it was, that the case was accurately described and the dates carefully given. Had the case fallen into the hands of a medical man of less acumen, or one who had unlimited faith in mercury, undoubtedly that drug would have got the credit of causing a complete cure. It was by a series of such cases that the scientific study of syphilis would advance. A haphazard mtehod of administering drugs would prove anything, and keep up faith in medicine. He felt sure the Society was indebted to Dr Cadell.

II. *Mr Bell* then read a paper on PARAFFIN EPITHELIOMA OF THE SCROTUM, which will appear in a future number.

Mr Chiene was glad that Mr Bell had brought forward his instructive cases, and in his short, curt way given them the interesting points. He regretted, however, that allusion had not been made to the importance of these cases in reference to the etiology of such diseases. A consideration of English chimney-sweeps' cancer, and the cases just given, lead to the supposition that, in those with a tendency to these diseases, any irritation would start it. He was not aware that the English coal was the cause of the cancer, but had always believed that it was due to the employment of climbing-boys there, and therefore greater exposure to the effects of the soot. As to the paraffin so acting, it appeared to be as Mr Bell said, viz., that the scrotum was a rugose part, and the dirt lodging there caused irritation. Another interesting point was, that though many workers were exposed to the same influences, in only a few did the cancer occur. If in the cases given the history could have been traced, and the fact of any relations being affected by like diseases satisfactorily shown, the interest of the paper would have been much increased.

Mr Bell wished only to supplement what Mr Chiene had so well said. In a previous communication as to the occurrence of epithelioma of the lower lip in women, he had stated that in seven such cases it had been noticed that all were smokers, and his theory therefore was, that the short black pipe used acted as a local irritant. If he had only twenty years' more experience, he hoped to make a most interesting paper on this and allied points regarding the etiology of cancer. In both the cases forming the subject of his paper, inquiry had been made, and it was alleged that no similar affection had existed among the relations.

Dr A. Robertson wished to make one remark. He saw how in the case of the chimneysweeps' cancer, the soot might act as an irritant. But in regard to the paraffin vapour, he did not see how this could be. The men were protected by woollen clothes, which would effectually stop any vapour; and therefore, while he could believe in the accredited origin of the soot-cancer, he was not so clear as to that of the paraffin form.

Dr Matthews Duncan pointed out that Dr Robertson had omitted what he thought an important fact, viz., the frequent occurrence of a skin eruption on the parts exposed to the action of the paraffin. This showed it was a powerful irritant. He would also like to remark on the extreme difficulty in getting reliable information as to hereditary disease.

Dr Bruce, as explanatory to Dr Robertson, narrated an instance of a boy who came into his study, and had such an overpowering smell of paraffin, that he was glad when he left it. His clothes seemed to be so thoroughly saturated, that Dr B. could easily understand how any part of the body liable to be injuriously affected by the fumes would have the opportunity afforded to it.

Mr Bell explained that the theory of the paraffin-workers was, that an eruption appeared on the scrotum just as on the hands, etc., and that the consequent itchiness caused them to scratch it. As this was done with their dirty hands, the paraffin got direct access.

Dr T. A. G. Balfour thought that in reference to the subject of paraffin, it might be interesting to Mr Bell to know that many years ago, he, Dr Balfour, had been informed by a medical friend in the neighbourhood of Bathgate, that that substance, when rubbed over the body, was the best cure for scabies; though, of course, there was a great difference in the cause of this disease and that of eczema, to which allusion had been made.

III. *Dr Littlejohn* then read a paper on THE COMPULSORY INTIMATION OF DISEASE, which will appear in a future number.

Dr Andrew Wood said that when he got the billet of proceedings, and saw that there was a paper on such an important subject, he felt anxious to come and hear what was to be said regarding it. In the present day, all had come to the grand conclusion that the prevention of disease was better than its removal, and that, to the attainment of that most desirable object, the energies of the medical profession, aided by sound sanitary legislation, should be vigorously and continuously applied. During the last few years, for example, great progress had been made as to typhus and typhoid fevers. In now looking back on a long career, and considering how fever raged in the earlier years of his practice, laying many a breadwinner low, and sending his family to the poorhouse, he could see how different it was at the present day. Last year, during sixteen weeks, there had been actually no death in Edinburgh from fever. On such a result they might well congratulate themselves. It was greatly owing, no doubt, to improvement in the condition of the working-classes, to better food and more leisure, as well as to more efficient drainage. He also believed that, without flattery, much was due to the action of their indefatigable Officer of Health. He was a man not easily daunted, and when he once set out on a course which approved itself to his mind, nothing would hinder him from bringing it to a successful conclusion. As to scarlatina and hoopingcough, it was painful to see the bills of mortality, especially as to the former. The mortality from scarlatina was exceedingly large; it was a disease that could not be rooted out like typhoid fever, for example, as it was dependent, not on drainage, but on a specific contagion. It was only by isolation, and by destruction of fomites, that we could hope to diminish its ravages. Dr Littlejohn had shown that this continued prevalence of scarlatina was due to the culpability and carelessness of many, not only in the lower, but also in the upper classes, who, with remarkable elasticity of conscience, seemed to care little for the prevention of the disease as affecting others. But Dr Littlejohn had also suggested a remedy for this. In considering this remedy, we should remember what was done in the case of cholera when it last visited this city. It was really nipped in the bud by the vigorous adoption of sanitary measures. The profession generally would agree with Dr Littlejohn's suggestion. The plan of schedules was very good, and must, to be effective, be carried out by the medical attendant, as the parents could not be depended on. The remuneration was certainly small; but this should not stand in the way of their doing their duty and to obviating the spread of disease and death. We had already done too little in endeavouring to stamp out scarlatina and other diseases. We certainly could not vaccinate as in smallpox; but at any rate isolation could be more readily enforced, bedding destroyed, and other precautions taken with good hope of success. The Society, as well as the community at large, was much indebted to Dr Littlejohn for bringing such an important matter before them. As to the details and compulsory powers, there might be a difference of opinion; but there should be none in helping to carry out a scheme which seemed so desirable, and which, if successful, would earn for Dr Littlejohn the gratitude of his country in a most remarkable degree.

Dr Matthews Duncan entirely agreed with Dr Wood in his eulogium on the paper, and the energy, ability, and utility of their excellent Officer of Health. The most difficult point was—Who was to give information? Dr Littlejohn said that it should be the medical attendant. He, on the contrary, held that the medical attendant should give no such report, unless compelled by statutory enactment. His notion of honour, in regard to the trust reposed in him by private patients, would prevent him. If, however, the law should compel him, then it was a different matter. But he would oppose the passing of such an Act. For while increase of knowledge in regard to zymotic diseases is desirable for medical men—and they would gladly help to make it more exact—yet with this view, it is wrong to ask them to report. The onus should be laid on the patient or his guardian. Many families indeed managed diseases, *e.g.*, measles, without any doctor, and many objected to his early presence on account of the expense; and in such cases to rely on the medical man as a spy would be a great mistake. In the matter of the registration of deaths, notice was given by those connected with the deceased. The same principle should be adhered to in regard to disease. Let those connected with the patient register the disease, and let the doctor, if there is any difficulty, state its nature. For this, of course, he should be paid. As to Dr Littlejohn's remarks on the grazier, it was certainly true that the latter was brought up among cattle. But so are parents among young children; and there is therefore no reason why a grazier should know more about his cattle than the people at large should know about theirs. A mamma, indeed, was more competent to judge of the illnesses of her children than a grazier as to those of his animals. In addition, the grazier knows well what money he is losing; but it was not so with human life, and on this account the subject was more advanced as to cattle diseases. This, however, was no reason why we should be less careful as to human beings than as to cattle. Then, as to the difficulties of legislation, Dr Littlejohn spoke of preventible diseases *en masse.* Why, the expense would be tremendous, as they were so numerous. The mere registration of such diseases as ringworm, gonorrhœa, syphilis, measles, scarlatina, etc., would be a small fortune to the medical man. But he further wanted to know what was the good of registration of preventible disease. The main good (Dr D. said) would be increased knowledge of such diseases. For the prevention of disease no direct or immediate results could be predicted. They had heard to-night rash statements, such for instance as nipping cholera in the bud. He did not believe in this. We were too ignorant of the causes of the spread of such diseases to be able to act with much efficiency. In one of the largest towns of this country, with certainly one of the ablest sanitary officers in charge, the corporation had been incessantly adopting all his schemes at a vast expense, with only the result that the mortality increased in spite of them. The reason was simply that we did not know sufficient as to the diffusion or prevention of disease. All that could be certainly expected from any such measures as Dr Littlejohn proposed would be simply to increase our knowledge. The whole gist of this is seen in the case of scarlet fever. It has the pre-eminence of being a most fatal disease. Now, the popular notions as to it were quite erroneous. Thus, scarlet fever was said, even by the profession, to be specially dangerous to lying-in women, as causing puerperal fever. At the last meeting of the Society, however, he had shown tables of mortality for thirty years. By these it was proved that, with 250 weekly deaths from scarlet fever, there were in London no more than usual from puerperal fever, although many of the lying-in women must have been attended by practitioners who were at the same time attending scarlet-fever patients. Now, if legislation proceeded on such erroneous grounds, the result would be that they would have an endless number of useless restrictions. Another belief was as to doctors frequently diffusing scarlet fever. He had never done so: none of his children had yet suffered from it. The same was the case in other medical men's families who saw more of such diseases than he did. But the case of scarlet fever was still more to the point. From Dr Littlejohn's expressions, it might be thought that an epidemic started up in a moment. This was not the case. An epidemic of scarlet fever began slowly, and the increasing mortality would show when it began to be epidemic. In conclusion, he would say that if they studied epidemics they would find they went on irrespective of medical men, who could do very little as yet to modify or prevent them. But while he held the opinions he had expressed, he would yet support Dr Littlejohn's scheme as a whole, with a view to at least increasing our knowledge of disease.

Dr Wilson had considerable reluctance in rising. He would not like it, however, to go forth to the public that we knew so little as to scarlet fever. He held that, though one of the most infectious, it was one of the most preventible of diseases. In patients among the better ranks they could, by putting the patient to the top of the house, and by removing the cuticle by bathing, and subsequent disinfection of the clothing, etc., effectually prevent the spread of the disease. He had some difficulties as to any penalty being inflicted; but he thought that no child should be allowed to go to school during the desquamative stage, indeed for six weeks, under a penalty.

Dr Ballantyne thought that the registration of the weekly morbility of paupers by parochial medical officers all over the country would be valuable as indicating the rise and progress of epidemics; the Board of Supervision making a contract with the medical officers to furnish such a report at a moderate fee. It would be a good preliminary measure, against which no objections such as those adduced by Dr Duncan could be urged. It was undoubtedly difficult to say when the danger of infection began and ended. Thus, Burdon Sanderson had found that cattle disease was infective before any symptom but a rise of temperature showed itself.

Dr Bruce thought Dr Littlejohn's scheme of great importance, and one that, if carried out, would be very good. There were, however, great difficulties in the way of doing so. For instance, who was to give information? Some say that the medical man should. But there were many cases to which he was never called, unless for sequelæ. There were many other points to attend to, especially letting patients out while there was any risk of their communicating infection. Many did not care if other children were hurt, provided their own came to no harm.

Mr Chiene wished to know some definite particulars as to how Dr Littlejohn would proceed.

Dr Littlejohn was gratified with the reception his paper had had, and with the discussion that had followed. As to Mr Chiene's query, he would, if requested, visit the infected house—if need be, remove the patient, and take care that infected articles of bedding and clothing were disinfected. If there were other children in the house at school, he would prevent them continuing their attendance, and he would communicate with the schoolmaster. Should parties, liable to come into close contact with the sick, be engaged in workshops or saleshops, he would certainly take steps to prevent their carrying infection. He differed from Dr Duncan as to the lessons to be drawn from the picture which he drew of our helplessness in presence of epidemic disease. The true principle was *obsta principiis*, and he was convinced that were epidemic diseases promptly dealt with at their commencement, they would not commit the ravages with which we are all conversant, and which were the *opprobrium* of medicine. We were not powerless in the presence of epidemics, and he could refer to the manner in which smallpox and cholera had been successfully met, as evidence of the benefits of preventive medicine. So long, however, as the medical profession limited themselves to simply treating the case of illness to which they were summoned, and did little or nothing in the way of preventing the spread of the disease, we would suffer from the ravages of epidemics, which were more costly than Dr Duncan appeared to regard them. The calculation has been frequently made, and it was startling to see the large expense an outbreak of any infectious disease entailed on the country. It was easy for Dr Duncan, from his experience among the better classes, to throw doubt on the highly infectious nature of some of the diseases referred to in the paper. But the experience of sanitary officers was that, in the miserable overcrowded dwellings of the poor, these diseases spread like wildfire, and quickly assume an epidemic form. Dr Duncan's argument from the registration of deaths, that the proper party to give information should be the nearest relative, was not to the point. A delay of eight days was allowed in the case of a death, and weeks generally elapsed before the medical attendant sent in his schedule; but days and hours were of importance in dealing with infectious diseases, and there could be no doubt that the sooner the information was received, the more potent were the means at our disposal to check the progress of such ailments.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXV.—MEETING VI.

Wednesday, 9*th February*.—Professor SIMPSON, *President*, *in the Chair*.

I. *Dr Matthews Duncan* read a paper on RUPTURE OF THE PERINEUM, AND ESPECIALLY ON THE DIVISION OF THE PERINEUM AND RECTOVAGINAL SEPTUM, BY POST-PARTUM SLOUGHING, which appears at p. 865.

Prof. Simpson considered the last case related by Dr Duncan to be of great interest, and quite unique, as in his experience he had neither met with, nor heard of, a similar one. The second case was a more ordinary one, in which, through great pressure, bruising and laceration of the mucous membrane and cellular tissue in the floor of the pelvis had taken place to such a degree that the extravasated blood appeared under the skin of the perineum at the point where subsequent softening and solution of continuity of tissue had resulted. He referred to a case of rectovaginal fistula which had come under his observation, in which he had found a difficulty in deciding whether, in the first instance, the rupture had been complete or partial, a portion having subsequently healed. In this case he had succeeded in closing the wound by the same kind of operation as that performed in Dr Duncan's case.

Dr Angus Macdonald, through Dr Duncan's kindness, had seen the last case related in Dr Duncan's paper. He was quite at a loss to account for its genesis.

Dr Milne thought the case most difficult of explanation.

Dr Duncan allowed that the case was a very peculiar one, and in some respects almost incredible. Had he not verified the facts by his own immediate observation, he might have doubted their correctness. He had lately met with a case in which extensive rupture of the perineum short of the anus had taken place, and the whole healed without any treatment whatever.

II. *Dr Underhill* read a paper entitled A CASE OF ABSENCE OF THE UTERUS, WITH REMARKS, which appears at p. 906.

Dr Matthews Duncan considered the tumours which were felt in either groin to be ovaries, and not testicles. This view was supported by the fact that the patient had undoubted sexual appetite for the male. He had frequently met in practice with cases in which the vagina ended in a *cul-de-sac*, and no uterus could be found. He had lately seen two cases of this kind in sisters.

Dr Milne also was inclined to think the little tumours were ovaries, not testicles.

Dr Angus Macdonald thought that the natural up-bringing and external conditions might powerfully influence the development of sexual desire and proper feeling of sex.

Prof. Simpson did not think that sexual appetite alone could determine the sex. Women who had been ovariotomized had still sexual appetite. In regard to Dr Underhill's case, he thought it probable the individual might be a male as to internal generative parts. It was quite possible, in cases where no uterus could be detected per vaginam, to establish a diagnosis of the character and position of the internal generative parts by making a combined examination through rectum, hypogastrium, and bladder. He thought it desirable that further examination should be made in this way.

Dr Underhill having briefly replied, public business was concluded.

Part Fourth.

PERISCOPE.

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal.*]

Bromhydrate of Quinine.—Gubler arrives at the following conclusions relative to the therapeutic effects of bromhydrate of quinine. It is more soluble and richer in alkaloid than the sulphate. It possesses the physiological properties of the salts of quinine generally, and probably also the therapeutical virtues of its officinal congener. It differs in action from the sulphate, not only by inducing less quinine intoxication, but also by producing a marked tendency to nervous sedation and hypnotism. It is especially indicated in the treatment of congestive and febrile affections of the nervous system; neuralgia, neuritis, irritative neuroses, encephalic hyperæmia, etc. It has manifested remarkable power in cases of obstinate vomiting, and has been of service in cases ordinarily amenable to the sulphate, as in visceral and articular fluxions of diathetic, rheumatic, or gouty origin, symptomatic fevers, etc. It is given in quantities of from six to fifteen grains per diem, in doses of three grains in the pilular form, or hypodermically injected. In no case has the hypodermic injection been followed by any inflammation. The complete innocuity of the bromhydrate of quinine, and its greater solubility, render it immeasurably superior to the sulphate for hypodermic administration.—*Jl. de Thérapeutique,* 10th July and 10th September 1875.

Copaiba.—Dr S. Wilks first suggested the use of copaiba as a diuretic. He has a paper in the *British Medical Journal,* 25th December 1875, in which he states that his further experience of it has confirmed his early opinion of its great value. The following formula is used in Guy's Hospital:—Copaibæ resinæ, ʒi.; spt. rect. ʒi. ss.; chloroformi, ʒi.; syrupi zingib., ℥ss.; mucilag. acaciæ, ℥i.; aquæ ad ℥vi. A sixth part of this is given thrice daily. This mixture is not nauseous, and has scarcely an odour of copaiba. Its action is almost immediate, the first or second dose displaying the diuretic effect. In bronchitis it is probably the oil which acts beneficially, while in gonorrhœa the diuretic properties of the resin and the specific properties of the oil are both advantageous, and hence the ordinary balsam should be employed.

In the *Practitioner* for February 1875, Dr E. L. Dixon records three cases of dropsy in which great benefit was produced by the administration of copaiba. The drug seemed to act by increasing the amount of urinary water, for the specific gravity fell as the amount of water increased. Two or three of Durant's capsules, each containing from five to ten minims, were usually given night and morning. Dr A. R. Hall, of the Army Medical Department, has a paper in the *Practitioner* for April 1875, on the therapeutic value of balsam of copaiba. He recommends it in iritis, sclerotitis, and as a topical application in purulent ophthalmia. He contends that the advantages which apparently follow its use in almost all cases of inflammation of the eye are not generally recognised. In threatened mammary abscess, he suggests the local application of copaiba, and he records a case illustrative of its efficacy in this affection. In the order urinæ of gonorrhœa, in bronchitis, and in chronic cystitis, when the urine contains much mucus, copaiba is of great service. Dr Hall thinks that the balsams act beneficially in inflammation, partly by their effects on the venous system and partly by their actual presence in the bloodvessels, stirring up the capillary circulation when congestion has commenced, and preventing the formation of points of stagnation or centres of suppuration.

Dr F. J. Brown states, that balsam of copaiba, with equal parts of castor-oil and glycerine of carbolic acid, forms an excellent application to callous ulcers. —*Practitioner*, April 1875.

DIGITALIS. — (*a.*) *Diuretic Action.* —Drs Brunton and Power show that the diuretic action of digitalis is not due to its power of increasing the blood-pressure in the arterial system. They assume that digitalis stimulates the vaso-motor nerves generally, but affects those of the kidney most powerfully. It thus causes excessive contraction of the renal vessels so as to stop the circulation and arrest the secretion of urine. As the action of the drug passes off, the vessels relax, and the renal arteries dilate more quickly and to a greater extent than the others. It is possible also that alterations in secretion may be partly due to the direct action of the drug on the secreting elements of the kidneys.—*Proc. Royal Soc.*, xxii. p. 420.

(*b.*) *In Typhoid Fever.*—Mr W. Murrell, in the *Practitioner* for November 1875, relates particulars of a case of typhoid fever which suddenly terminated fatally during its treatment by digitalis. The practical lesson to be learned from this case is, that although by means of digitalis we may rescue a patient from the deleterious influence of an excessive rise of temperature, yet that its use is not unattended with danger, and that during its exhibition death may occur suddenly and unexpectedly, and without a single note of warning. Mr Murrell reviews the various opinions held as to the mode of action of the drug.

(*c.*) *In Maniacal Excitement.*—In the *Jl. of Mental Science* for January 1876 allusion is made to Dr Bigot's researches relative to the action of small doses of digitalis in maniacal excitement, chiefly of epileptics. Ten-minim doses of the tincture were given twice daily. In many of the cases the treatment was eminently successful.—*Annales Médico-Psychologiques.*

PICROTOXINE.—M. Gubler lately tried picrotoxine in a case of labio-glosso-pharyngeal paralysis. A subcutaneous injection of $\frac{1}{70}$th grain in solution was administered. After the administration of the drug deglutition could be accomplished, and the patient was enabled to pronounce words distinctly. The latter improvement was, however, not permanent. The injections, although not painful, caused induration of the cellular tissue.—*Bull. de Thérapeutique*, 30th Nov. 1875.

Povergo has shown that the action of picrotoxine and strychnine on the inhibitory centre of the cord are similar.—*Schmidt's Jahrb.*, clviii. p. 130.

M. Dujardin-Beaumetz has tried picrotoxine in epilepsy. Commencing with less than a milligramme per diem, he ultimately attained to three milligrammes daily. A prompt amelioration took place in the symptoms, and the fits ultimately disappeared altogether.—*Gazette Méd. de Paris*, No. 51, 1875.

MORPHIA.—(*a.*) *Subcutaneous Injection.*—Alling recommends the following formula:—Hydrochlorate of morphia, one decigramme; distilled water, four grammes; cherry-laurel water, one gramme.—*Alger. Medicale*, No. 24.

(*b.*) *As a Palliative in Chronic Bright's Disease.*—In the *Practitioner* for October 1875, Dr R. Park gives details of a case of chronic Bright's disease, in which he employed morphia subcutaneously as a palliative with considerable benefit. Dr Potter has also treated a case of uræmia successfully with morphia injections (*Boston Medical and Surgical Journal*, 27th May 1875). Dr Loomis, in his recent work on Diseases of the Respiratory Organs, Heart, and Kidneys, advocates a similar treatment.

(*c.*) *In Spasmodic Asthma.*—Dr J. Keith Anderson records that on twelve occasions in which he employed subcutaneous injections of morphia in spasmodic asthma, the result has been a complete and perfect relief from the embarrassment of the respiration. The dose he used in all cases was one-sixth grain of the hydrochlorate in a strong solution. In no case was there a tendency to the recurrence of the breathlessness after the first effects of the morphia had passed off.—*Practitioner*, November 1875.

(*d.*) *In Obstinate Constipation.*—In the *Practitioner* for February 1876, Dr W. M'G. Burns has a brief communication on the value of subcutaneous injections of morphia, not only in spasmodic asthma,

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Complete Evacuation of the Uterus after Abortion.* By Professor ALEXANDER RUSSELL SIMPSON, President of the Edinburgh Obstetrical Society.

(*Communicated to the Obstetrical Society of Edinburgh*, 8*th March*.)

IT is my purpose in this communication to draw attention to our means of securing the complete evacuation of the uterus, in cases of abortion and miscarriage. Interruptions of gestation in the early months are very common; but while the authors of our obstetric text-books are careful to point out the evils that may result from the detention of fragments of ova within the uterus, they fail to explain how we can best clear out the cavity.

It is extremely difficult to determine the frequency with which abortions occur. Not to speak of the cases of wilful provocation and concealment of them, they may take place in the early weeks in patients who suppose they have merely a delayed and perhaps profuse menstruation. Even when women have supposed they were pregnant, they may think lightly of it when hæmorrhage sets in, and may cast the fruit of the womb at an early stage, without desisting from their ordinary pursuits, or, at least, without calling to their aid a medical practitioner. Hence the diversity, in the estimate given by different writers, as to their relative frequency; Hegar[1] reckoning at least one abortion to every eight or ten full-time deliveries, while Devilliers[2] sets them down in the proportion of one to three or four. The statistics of Whitehead[3] show a proportion of about one to seven. He takes care, however, to draw attention to the fact that the average age of the women whose history he had ascertained, was a year and a half below the middle of the child-bearing period; and that while thirty-seven out of every hundred mothers experience abortion before they attain the age of thirty, the percentage of those living on in wedlock till the menopause who are subject to this accident rises as high as eighty-seven.

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[1] Monatsschrift für Geburtskunde, XXI. (Supplement), 34, 1863.

[2] Nouveau Dictionnaire de Médecine et de Chirurgie, IV. 305. 1866.

[3] On the Causes and Treatment of Abortion and Sterility, p. 245.

The evil effects of an incomplete abortion are either immediate or more remote. The great immediate risk is the occurrence of excessive hæmorrhage, which, though but rarely fatal, is sometimes very alarming, and always leaves the patient in a state of deteriorated health. Then apart from the chances of decomposition of the retained fragments, or of the discharges that attend it, and the possibility of septic absorption from the surface where separation may be taking place, the uterus which contains a foreign body remains decidedly hypertrophied, so that when it finally becomes evacuated, the walls may remain permanently thickened and the cavity enlarged. In a patient with the uterus in such a condition, dislocations of the organ easily occur. Such imperfect involution may easily be the starting-point of other morbid changes; and thus it comes about that many of the women who come under observation suffering from uterine affections can trace back the commencement of their distress to an abortion in the early months of gestation.

When we have to do with a case of abortion in which the stage of expectancy is clearly over, and the patency of the os internum, or the persistence of the pains, or the hæmorrhage long continued or profusely flowing, call for active interference, there are two main indications to be fulfilled, viz., 1st, to restrain the hæmorrhage, and, 2d, to procure the perfect removal of the ovum.

I. *Restraint of Hæmorrhage.*—In treating of the fulfilment of the former of these indications most of our manual writers are satisfactory enough, though even under this head there is some room for improvement. In a typical case we place little reliance on rest, cold, styptics, and so on, for we know that we can only effectually restrain the hæmorrhage either (1st) by compelling the uterus to more energetic contractions, or (2d) by blocking up the channel through which it escapes. In most cases we pursue both objects simultaneously.

1*st*. The uterus must be compelled to steady action by the administration of ergot, introduced in the form of a strong solution of ergotin[1] into the subcutaneous cellular tissue—a mode of administration of the drug which is destined, I believe, to supersede those with which we have so long been familiar; for the subcutaneous injection of ergotin sets up the uterine contractions with such speed, such certainty, and such safety, that the hypodermic syringe will be found an indispensable part of the furnishing of every obstetric bag.

2*d*. Pending the onset of effective pains, and with the view of at once hastening them and preventing the escape of the blood, we plug the genital canal. For the most part, and as if it were the best in all cases, authors recommend the use of the vaginal tampon. Now, the plugging of the vagina, by whatever material, is sometimes the only expedient to which we can have recourse, and we may rely on it with much certainty for averting the immediate danger of excessive hæmorrhage. But it seems to me that, where we have our choice, its application ought to be restricted to those cases where there is still some hope that, if we succeed in arresting the hæmorrhage, we shall at the same time succeed in averting the impending abortion. Otherwise we have a means of controlling the bleeding more directly, at once less irksome to the patient, and less troublesome of application by the practitioner, in the cervical plug. For this purpose there is nothing so efficient as a good-sized sponge-tent, prepared, as Messrs Duncan, Flockhart, and Co. now always prepare them, by being dipped in a disinfectant solution before being compressed by the whipcord. In my judgment, nothing can take the place of the sponge-tent for rapid, kindly, and complete dilatation of the cervical canal. And when we have introduced a sponge-tent through the cervix uteri in a case of unavoidable abortion, we may count upon a threefold effect. *First*, The bleeding is arrested immediately and effectively; for if there be any escape into the uterine cavity, it has simply the effect of swelling up more rapidly the sponge, which blocks up its canal of exit. *Second*, The uterus is stimulated to more rapid and more energetic action, as it always is when a foreign body is introduced within its cavity. *Third*, The cervical canal is being effectually expanded during the lodgement of the sponge within it; and where a sponge-tent of good size has been well planted, it secures, as no other tent that I know of does, the expansion of the sphincter of the os internum. When, by the adoption of these measures, we have fulfilled the first indication, we have at the same time prepared the way for the carrying out of the next, viz.,

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[1] I use for hypodermic injection a solution of one part of ergotin in three of water, with ten or twelve grains of chloral in each half-ounce bottle of the mixture. Ten drops of this fluid gives the action of the drug with great certainty, and I have seen no ill effects from it.

II. *The Complete Evacuation of the Uterus.*—It is in describing the methods of securing this object that I find systematic writers particularly defective. For the most part, they tell us that when the uterus fails to expel its contents, these must be extracted with the finger or fingers passed through the cervix, and hooked round the ovum or placenta, or whatever fragment may still be *in utero;* and that, when it cannot be reached with the fingers, cautious use should be made of some forceps or other instrument. But then the methods by which the uterine cavity can be reached through the vagina are hardly more than hinted at, with a few exceptions, as by Cazeaux, who, in a single line, speaks of "strong pressure upon the hypogastrium to depress the womb;" and Schroeder, who, somewhat more fully, describes the bi-manual manipulation. If, *e.g.*, we open the latest work, the deservedly popular "System of Midwifery" by Professor Leishman, we read[1]—"The finger is to be cautiously passed round the protruding portion, and, if necessary, another finger may be introduced into the os. If we can thus succeed in getting a hold of the placenta upon which we can rely, it may be extracted entire; but a rude or unskilful mode of manipulation may entirely frustrate our efforts by leaving behind a portion of what we wish to extract whole. It is impossible to lay down rules for the skilful performance of this manœuvre, which can only be taught by experience; but we have no doubt that more reliance is to be placed upon the fingers than upon instruments, as a general rule." It is little wonder if, a page further on, we find him speaking on the after-treatment (p. 434) of fragments of placenta giving rise again to hæmorrhage; and all that is said of the treatment in that case is, that "the patient must not be permitted to rise until all trace of this has ceased!" Why, the abortion in that case is unfinished, and we have no right to expect a cessation of the hæmorrhage until the delivery has been complete. It always seems to me that there are few obstetric manœuvres capable of clearer exposition than the manipulations that we employ for emptying the uterus in the last stage of an abortion. Let us first have a clear idea as to what is required to be done. We have to pass one or more fingers into the cavity of the uterus to explore the entire cavity, to separate from its walls any adherent portion of the ovum, and then to extract the separated mass. I say at once, and simply, that it is the fingers of the operator that are to do the intra-uterine work, for I am quite at one with those who deprecate the use of instruments, such as curettes, wire-loops, crotchets, and abortion forceps, for the *detachment* of retained ova or their fragments. When the adhesions have been all separated by the finger, it may sometimes facilitate the removal of the loosened body to seize it with such instruments, and in that case, a pair of long dressing forceps, or polypus forceps, suits as well as any special implement; but the detaching of the adherent portions of the placenta should in no case be intrusted to these, or attempted with them, seeing that it can always be effected by the use of the sense-guided finger.

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[1] Second edition, 1876, p. 433.

But how are we, with the finger or fingers, to reach so high in the cavity of the uterus as completely to surround the ovum? To begin with, the patient should, as a rule, be anæsthetized. The manipulations necessary to secure a satisfactory result cause suffering, though not to a great degree, which we can always save the patient by bringing her under the influence of chloroform. And at the same time that her sense of pain is abolished, her voluntary muscles are completely relaxed, and it becomes easy for the practitioner to press down the uterus through the abdominal parietes. Once and again I have found myself baffled in the effort to reach the fundus uteri in such a patient until I had chloroformed her; for, however willing the woman may be to further your efforts for her delivery, involuntarily she contracts the recti abdominis when you make pressure on the hypogastrium, or withdraws herself when you press the other hand against the perineum. The patient, then, having been anæsthetized, we may render the uterine cavity accessible to the exploring finger in one or other of two different ways.

1*st*. We can push down the fundus uteri from above. The patient may lie either supine or in the ordinary obstetric position on her left side, with the knees drawn up; most frequently the right hand will be used for internal manipulation, while the left is applied to the abdominal surface. It rarely suffices to pass one finger alone into the vagina. In most cases the index and middle fingers are passed into the vagina, and while the middle finger is folded in the fornix to steady the uterus there, the forefinger is passed through the cervix. Or the middle finger can sometimes be more satisfactorily employed for the intra-uterine digitation, or, better still, both fingers may be passed into the uterine cavity. In the last case it may become necessary to have the other two fingers carried into the canal of the vagina, the thumb alone remaining external to the vulva. It is usually only in patients who have miscarried at the fourth month, or beyond it, that the hand requires to enter so far for the separation of the placenta, and then the vaginal cavity is relaxed and roomy enough easily to permit of it. Whilst the fingers of the right hand are thus seeking their way up to the recesses of the uterus, the left hand, applied above the brim of the pelvis, is pressing the uterus forcibly and steadily downwards into the pelvic cavity. In this way, in the great proportion of cases, we obtain perfect command of the uterine contents. The fingers of the two hands recognise each other through the double thickness of the abdominal and uterine parietes; and while the left hand keeps the fundus fixed firmly downwards, the forefinger of the right peels off the adherent mass and forces it through the cervical canal. In the great proportion of cases, I repeat, we can in this manner compel the evacuation of the uterus, and when it fails us our resources are not yet at an end, for,

2*d*. We can drag down the cervix from below. The first is the method that has most frequently been employed, and it has this in favour of its common employment that abortions are more frequent in multigravid than in primigravid women; in women, therefore, in whom there is usually a degree of abdominal relaxation, which greatly favours its execution. But where the walls are more resistant, or the patient is so fat that the combined external and internal manipulation fail us, then we must seize one or other of the lips of the uterus—usually the anterior—with a volsellum, double or triple pronged, and slightly curved. One of the blades grasps the vaginal aspect of the front wall of the cervix as high up as the roof of the vagina, the other at a corresponding level within the cervical canal. The uterus is capable of being dragged far down without any injury to its ligaments or laceration in the bite of the volsellum. It may be pulled down with the right hand and kept fixed with it, whilst the fingers of the left pass into the cavity and explore and evacuate it. Or the volsellum may be held in the left hand, or given to an assistant, to keep the uterus depressed, whilst the more familiar right-hand fingers do the intra-uterine work. The cavity of the uterus is thus brought within full reach of the fingers, and we can—and in all those cases of imperfect delivery in the early months we ought to—control the emptying of the cavity from fundus to os.

Whilst the method of gaining access to the interior of the uterus by pressing it down from above is that which has hitherto been ordinarily followed, my own experience leads me to expect that this second method, which I have just described, will largely supersede it. For, first, it is applicable in all cases where the other can be employed, and in some where the rival method is not available. Second, it is less painful, and may be carried out occasionally when there is not time for the administration of an anæsthetic. Third, it saves the expenditure of muscular power demanded of the practitioner, who presses and keeps the uterus pressed down from above only by overcoming the resistance of the abdominal walls. The one circumstance that will enable the bi-manual method to hold its ground is, that we may find ourselves called on to clear out the uterus at a time when we have no volsellum at command, whilst our hands we always carry about with us.

I close by reading the notes, from my ward journal, of a case which I had under treatment quite recently in the Royal Infirmary, the occurrence of which, indeed, suggested to me the desirability of making this brief communication on a most important subject :—

CASE.—*Imperfect Abortion at an early month; Continuous Hæmorrhage, and Dilatation with Sponge-tent ; Removal of Fragment of Ovum ; Recovery.*

A. R., æt. 39, married, recommended by Dr Alexander of Leslie to the Infirmary ; was admitted 14th February 1876 into Ward XII. medical, complaining of a red discharge from the vagina, which has lasted nine weeks.

History of present attack.—The discharge began as an ordinary catamenia, and at first did not excite any alarm. Patient was nursing her last baby at the time (a child of fifteen months) ; but when she found the discharge still continuing to flow beyond the period of an ordinary catamenia, she stopped nursing. The discharge was then accompanied by considerable pain, of a bearing-down character, situated in the inguinal and hypogastric regions. This continued for about five weeks. Since then the patient has been free from pain. She did not observe any shreds or clots in the discharge ; but at the end of December (three weeks after its commencement), while sitting one day at the fire, she was aware of a body suddenly coming away from her, which she describes to be of a firm consistence, dark red, and about the size of an egg. Two other smaller bodies also came away, and the discharge for the moment was very copious. It has latterly been diminishing in quantity and becoming lighter in colour ; but last Wednesday (9th February) it was as bad as ever, and two small bodies like blood-clots came away.

History of previous health.—Is good, but patient has been falling off since the discharge began.

Obstetric history.— Patient has been married seven years, and has had four children. One is dead, the others are all healthy, the eldest being about six years old. All the children were carried to the full time. The labours were easy, with the exception of the last but one, which was protracted. On that occasion the lochia was very abundant, and a few weeks after, patient suffered from pain in the back. On applying for medical aid, she was told there was ulceration of the womb. There is no history of miscarriage.

Menstruation began at the age of twenty. It has not been attended by pain or undue discharge.

Present condition.—Patient is a fairly-developed woman of average height. There is no expression of suffering or obvious emaciation. Integument natural. Temperature normal.

Abdomen.— On inspection, a dark line between the umbilicus and pubis is seen, and lineæ albicantes. There is no tenderness on palpation. The percussion note is tympanitic.

Vaginal examination.—The ostium vaginæ is patulous, walls relaxed and very moist, at lower part of anterior wall a rough puckered surface is felt. Os uteri is rather high, is transverse, and directed slightly backwards; the anterior lip is thick, and some irregularity is felt on both lips. The finger can be passed through the cervical canal and internal os, but is immediately met by a round body of soft consistence, which seems to project from the uterine wall.

Diagnosis.—Incomplete abortion.

15*th February.*—Temp. 98°, pulse 76, resp. 20.

16*th February.*—Ordered hypodermic injection of ergotin.—*Vesp.* A large sponge-tent was introduced.

17*th February.*—An examination with a view to operation was made to-day under chloroform. On removing the sponge-tent, the mass could be felt projecting through the os. The anterior lip of the uterus was seized with a curved volsellum, and the uterus was pulled down and kept fixed in the lower part of the pelvis. Professor Simpson then was able to explore the entire cavity of the uterus with the forefinger, and peeled off without difficulty the polypoid body. This was adherent to the back wall of the uterus, somewhat low down and towards the left side; and whilst the patient was still asleep the surface of its attachment was distinctly felt by the resident physician and some members of the clinical class. The removed body proved, as had been anticipated, to be a fragment of an ovum. It was of the size of a walnut, and under the microscope the structure of placental villi in different stages of growth could be easily seen.

Vesp.—Had shivering in afternoon, skin moist. Ordered ergotin injection.

18*th February.*—Slept well all night. No discharge, no pain.

23*d February.*—A slight discharge the last day or two, but which is lighter in colour than before. Patient has no pain, and is looking well.

24*th February.*—On examination bi-manually and with the sound, the uterus is found to be of normal size.

25*th February.*—Patient discharged, well.

ARTICLE II.—*Case of Lymphatic Inflammation simulating Venereal Disease.* By FRANCIS CADELL, M.B.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 1*st March.*)

THE chief interest in this case lies in the great difficulty at first experienced in determining whether its nature was syphilitic or not.

The patient, aged 33, of fairly good constitution, had never before suffered from any form of venereal disease.

In the early part of June 1875 he had connexion with a prostitute, and three weeks afterwards he noticed, near the orifice of the prepuce, a small nodule like a corn seed, as he described it.

On the 9*th August* 1875, about five weeks after the appearance of the lump, I saw the patient for the first time, when the following condition was noted :—At the orifice of the prepuce, directly opposite the frænum, there was an indurated nodule the size of a small bean, its long axis being in the long axis of the penis. The general surface of this lump was red, with the exception of its most prominent part, where a small quantity of pus could be seen through its thin covering of cuticle. As yet, no ulceration had taken place. The consistence of the nodule was very similar to specific induration. The lymphatic vessel on the dorsum of the penis was indurated, and there was multiple indolent enlargement of the glands in both groins.

On 13*th August* the thin cuticle over the lump had burst, and a small quantity of pus had been discharged; there being no tendency to ulceration, the opening was scabbed over.

On 10*th September*, the tenth week of the disease, a second swelling on the dorsum of the penis had taken place. About an inch from the original lesion, and exactly in the middle line along the course of the lymphatic vessel, there was a rounded lump the size of a hazel nut. It had gradually increased to its present size. This second lump was connected with the first by the indurated lymphatic vessel, which felt like a crowquill.

The primary lesion has now the appearance of the mouth of a short sinus, and has continued to discharge a small quantity of sero-purulent matter.

14th September.—The lump on the dorsum of the penis was not so hard, and had diminished in size, having discharged a considerable quantity of pus through the sinus and external opening. No symptoms of constitutional syphilis have shown themselves. One of the glands of the right groin has suppurated.

13th November, the fifth month. Pus has been discharged, without intermission, until two days ago, when the orifice of the sinus was permitted to close. Swelling and œdema of the whole prepuce came on; this was, however, speedily relieved by opening up the orifice of the sinus with a probe, and allowing about a tablespoonful of thick yellow pus to escape. Since last report, two more glands have suppurated, one in the femoral region, and the other above Poupart's ligament, near the pubes. The patient's health has suffered somewhat from the long-continued discharge, but he has, nevertheless, since the commencement of the disease, been able to perform all his duties. There is still no appearance of any constitutional symptoms.

14th December, the sixth month of the genital affection. The discharge has now ceased, the orifice of the sinus is closed, and the lump behind it is reduced to the size of a small pea. There still remains some induration, but that is diminishing. The general health is good. The three glands, which suppurated on the right side, have all healed after an indolent course. No syphilitic symptoms.

29th February, the eighth month of the disease. There is a slight trace of induration at the seat of the original lesion still to be detected. The indolent enlargement of the inguinal glands has disappeared, and the patient's general condition is now quite good.

Remarks.—In two forms of venereal disease the lymphatic vessels of the penis are sometimes affected.

During the course of a gonorrhœa, the lymphatic vessels along the penis may be attacked, either with an acute or chronic inflammation, and abscesses may form along the course of the vessels, leaving sinuses very similar to what occurred in this case. Then, again, during the course of syphilis, the lymphatic vessels of the penis may become enlarged and indurated, like the lymphatic glands throughout the body. There is little or no tendency, however, to the formation of abscess, and it runs a chronic and indolent course, like the similar affection of the glands.

On first seeing the case, whose history I have just read, I not unnaturally came to the conclusion that I had to do with venereal disease of some sort, and most probably with syphilis.

The incubation period of three weeks which elapsed between the suspicious intercourse and the appearance of the local affection of the prepuce, the hard consistence of the primary nodule itself, and the multiple indolent enlargement of the inguinal glands, all tended to make me believe the case was one of syphilis. By the light of its history, however, there is every reason to believe that the lymphatic inflammation in this case was brought on by simple irritation, and was quite unconnected with any form of venereal disease.

What first made me doubt the syphilitic nature of the genital affection was, that when the skin broke on the most prominent part of the nodule, instead of a chancre-like sore being produced, the opening formed into the mouth of a sinus. Next, the long-continued and sometimes profuse suppuration was unlike what is met with in the early stage of syphilis. And finally, as month after month passed by, and no constitutional symptoms appeared, (no mercury having been administered), I felt convinced that, whatever the cause of the lymphatic inflammation had been, it was not brought on by syphilitic contagion.

ARTICLE III.—*Diphtheria: Typical Cases, in illustration of its Diagnosis, Prognosis, and Treatment; together with Remarks on the Signification of Albuminuria in that Disease.* By Sir JOHN ROSE CORMACK, K.B., F.R.S.E., M.D. Edin., M.D. Paris, Fellow of the Royal College of Physicians of London, Chevalier of the Legion of Honour, Physician to the Hertford British Hospital of Paris.

(*Continued from page* 906.)

CASE V.[1] *Pharyngeal Diphtheria; Formidable Paralytic Affections; Albuminous Urine; Recovery.*

On 21*st February* 1874, S. M., aged 49, a married Englishman, living with his wife and children at 6 Rue Antoinette, Belleville, Paris, applied to me in the consulting-room of the Hertford British Hospital for admission as a resident patient; and as such was forthwith received. Leaning on his wife, he was enabled to shuffle from the cab to the room in which I received him, a distance of about fifty yards. Some months previously, I had casually met him in one of the thoroughfares of Paris, when he seemed stronger and better than he usually appeared. This patient had been known to me for some years as an industrious man, struggling to surmount the evils incident to irregular employment. I made the acquaintance of this poor man and his family during the first—the German—siege of Paris, when, in common with about 1500 other British subjects, they were saved from starvation through the merciful ministry of the British Charitable Fund. In January 1871—the cruel month which preceded the capitulation—when icy winds were fraught with death, fuel a priceless luxury, rations at their lowest ebb, and "bread" had become a nauseous mockery of food, S. M. was exceedingly emaciated, and tottered like a feeble old man. It was not remarkable, therefore, that, in answer to my first question on 21st February 1874, "From what are you suffering?" he should at once have replied, "From a relapse of the siege disease." By careful questioning, a circumstantial history was obtained; but that history would not accurately present the whole case if unaccompanied by the introductory statement now made.

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[1] This case was read before the Medical Section of the British Medical Association at Norwich on 12th August 1874.

On 1*st January* 1874—that is, fifty days before he applied to me at the Hertford British Hospital—S. M. had begun to feel unwell, and three days later, he consulted a French physician residing in his immediate neighbourhood. He was then very weak, and suffered exceedingly from a swollen hand. His medical adviser prescribed cinchona wine internally, and for the hand, externally, a mercurial ointment. This ointment was used only for forty-eight hours, during which time the pain and swelling of the hand increased. He then, acting on his own ideas, discontinued the ointment, and applied a large poultice for twenty-four hours; at the end of which time he found that an abscess was pointing on the back of the hand. S. M. then applied to the poor-law doctor of the *arrondissement*, who opened the abscess, afterwards prescribed poultices, and then a lead ointment, which was applied twice a day for nearly a month. Under the free use of this ointment, the hand healed up, and returned pretty much to its normal appearance.

About the 20*th January*, when using the lead ointment, he became affected with a malady in the throat, similar to that from which two of his children were convalescent. This malady, from S. M.'s description, was evidently diphtheria, and was so called by the practitioner who attended him and his children. Neither of the children had any form of paralysis during, or subsequent to, their throat affections.

When S. M. had been put to bed on his admission, I examined him minutely, dictating at the time a complete report of that examination, which I now quote, as corrected at the bedside on the following day.

21*st February* 1874.—His lips are parched. His gums are rather spongy; they present no blue line. The tongue is thickly coated with a dirty, yellowish, sticky slime. On the mucous lining of the mouth, tonsils, and pharynx, there are aphthous patches, some of which are of considerable size. On the back of the pharynx there is a round deep ulcer, nearly as large as a half-franc piece, and covered with a mucous secretion.

There is a moderate amount of distension, and general tenderness of the abdomen. Besides the pain elicited by palpation and percussion, the patient complains of a distressing feeling of abdominal constriction, associated at intervals with a strong desire, but ineffectual attempts, to evacuate the bowels. He states that, during the ten days preceding admission, he has had only two stools, hard and scanty, the result of large doses of castor-oil, senna, and other purgatives, supplemented by copious enemata of salt and water.

At this stage of the examination, the patient became so weary and faint that I suspended my investigation of the physical thoracic signs which I had just begun; and, at the same time, I ordered some brandy and water to be administered as a restorative. When this mixture was held to his lips, he managed to get some within his mouth, but was unable to swallow any of it. Each attempt at deglutition induced spasmodic cough, the liquid which had been received into the mouth being at the same time ejected through the nose. Strong cold beef-tea, of the consistence of jelly, was then tried with rather better success. This description of food, however, likewise excited a choky cough; and with each effort at deglutition, while some was swallowed, more was returned by the nose as a liquid. Interrogated as to his dysphagia, he states that it has existed to some extent for nearly a fortnight, but it is only during the four preceding days that it has seriously interfered with alimentation. The increase of the dysphagia day by day was, he said, the immediate and urgent reason for his seeking admission to the Hertford British Hospital. When asked as to the kind of food which he could most easily swallow, he at once replied, " A morsel of meat or bread." He then proceeded to explain that he had been gradually losing the power of swallowing liquids, and seemed to have almost entirely lost it for the last two days. He added, that a bolus of meat or bread was swallowed with greater difficulty on the day of admission than on the previous day.

On examining the heart and lungs very carefully by auscultation and percussion, no morbid signs can be detected, except such as are distinctly imputable to debility. Neither the diaphragm nor the muscles of the thoracic walls are paralyzed.

One of the most striking features of the case at the first examination was the character of the voice, its weakness and nasality. After the examination had been continued for about a quarter of an hour the patient's answers were given in a scarcely audible and curiously nasal whisper. Indeed, the facts of the case as then reported could not have been rightly elicited, had it not been for the presence of the patient's wife. During the examination the pulse varied between 80 and 100; it presented no abnormal character except weakness.

Before proceeding with the narrative, it is well to state the tolerably precise conclusions which were at once arrived at from the facts now stated, in respect to diagnosis, prognosis, and treatment.

The *diagnosis* was *diphtheritic paralysis.* No doubt, the long-continued free use of the lead ointment suggested for a moment the possibility of the case being a mixed one—the conjoined result of galenic and diphtheritic poisoning. However, as the man had recently had diphtheritic sore-throat, and as he had no blue line on the gums, and as his symptoms were exactly those which often manifest themselves as a sequel to diphtheria, his case was set down as one of diphtheritic paralysis.

The *prognosis,* under certain reservations, was favourable. The unfavourable circumstances were the severity of the symptoms, and the prostrate condition of the patient, taken in conjunction with the fact that the period during which the paralysis had existed was still too short to warrant the hope that it had attained its maximum. Had the duration of the paralysis and debility been considerably longer, and had they been also no greater than they were, the unfavourable element in the prognosis would have been much smaller. The favourable prognostics were—that the diaphragm and respiratory muscles were unaffected; that there nowhere existed any recognisable organic lesion; and that, the debility of the patient being probably the result, to a great extent, of imperfect alimentation, it was likely to disappear under the judicious selection and careful administration of food, combined with the adoption of proper means for securing its digestion.

The *treatment* involved the adherence to important general principles, and the arrangement of numerous details in accordance therewith. Special treatment might or might not be tentatively tried at once; but in any case certain requirements were immediate and absolute, viz., faithful, intelligent nursing; suitable food, so administered as to obviate as much as possible the risk of choking; clearing out the torpid intestinal canal; and the use of means calculated to restore the mouth and pharynx to a wholesome state.

Special treatment, it was felt, might be advantageously allowed to remain undetermined for at least a few days, during which, perhaps, such guiding opportunities might possibly be detected, as are found not unfrequently in similar cases when carefully watched for from day to day.

With the view of emulging the liver and unloading the torpid bowels, a pill containing four grains of calomel was forthwith administered. It was likewise directed that two hours after he had taken the calomel pill he was to take another pill, containing four grains of the compound rhubarb pill of the British Pharmacopœia, and one grain of the extract of nux vomica; and it was likewise ordered that if, as was expected, no stool were obtained within six hours, as large a quantity as possible of salt and water should be slowly injected. The ulcer on the pharynx was brushed with pure creasote, and the mouth was directed to be frequently swabbed with the glycerine of borax, and washed out once in twelve hours with a weak solution of alum.

Next morning (*22d February*), at my ordinary visit about 11 A.M., I found the patient's strength decidedly increased, and his general appearance improved. The feeding had been well managed, and he had had some hours of refreshing sleep, from which he was roused by severe abdominal pain, which had been partially relieved by hot fomentations.

There was great tenderness on pressure all over the abdomen, and greater distension than on the previous day. A small quantity of urine had been passed, which, tested by heat and nitric acid, was found to be moderately albuminous. With the double view of relieving pain and relaxing the bowel, and in aid of the action of purgatives already taken, the abdomen was freely smeared with an ointment consisting of one part of extract of belladonna and three parts of lard. In applying this ointment gentle friction was employed. It was prescribed that in the event of the bowels not having been moved within six hours, two drops of croton-oil should be given.

On the following morning (*23d February*) I found the patient very weak, although he had had a good deal of sleep and aliment. He was, in fact, recovering from extreme exhaustion, which had supervened on an enormous unloading of the bowels, following the administration of two drops of croton-oil. The evacuation consisted of indurated fæces, slightly smeared with bloody mucus. During the passage of the fæces there was intense pain in the abdomen and at the anus. The blood observed was in very small quantity, and proceeded no doubt from hæmorrhoids. No medicines were ordered.

On *24th February*, the patient was greatly improved in countenance. He had neither pain nor uneasiness in the abdomen. There seemed to be some increase—there certainly was no diminution—in the paralysis and anæsthesia of the extremities. The difficulty in swallowing was less complained of, and was supposed by the attendants to be less than it had been since the patient's admission. The patient stated that his vision had been indistinct for the last two days—a fact which, when first mentioned, was attributed to the use of the belladonna ointment. There was some dilatation of the pupils. No medicines were ordered.

On *25th February* (the condition being the same as on the previous day), I prescribed a blister to be applied over the larynx and sides of the throat, feeling certain that the exudative stage of the diphtheria being long past, a safe time had arrived for making a vigorous trial of counter-irritation. I likewise prescribed two drachms of the compound tincture of cinchona to be taken three times a day in wine, with (in each dose) five drops of the liquor strychniæ of the *Br. Ph.* The rhubarb and nux vomica pill, full diet, and a daily litre of claret, were ordered to be continued.

Next day (26*th February*) there was a marked improvement in the patient's voice; it was considerably stronger, and much less nasal. This amelioration was probably due to the blister; at all events, I have seen blistering followed by a similar result in several similar cases.

From 26*th February* to 2*d March*, no record of symptoms was made—an evidence that nothing particularly noteworthy had occurred; and on 2d March there is simply a note to the effect that the patient was then complaining of "increasing blindness." The next report is full and important.

5*th March.*—For some days the patient has looked weary, and complained of great debility. His pulse is 84; his tongue is dry and coated. His bowels have not been moved for three days. Since 25th February he has taken two drachms of the compound tincture of cinchona, *Br. Ph.*, three times a day, with five drops of the liquor strychniæ, *Br. Ph.*, and at dinner a pill containing four grains of the compound rhubarb pill and one grain of extract of nux vomica. He has eaten abundantly till within two or three days, and has had a litre of Bourdeaux daily. In respect of the general appearance, there is marked improvement. The improvement in the voice and power of swallowing, which began immediately after the application of the blister, has gone on increasing steadily; and there now remains nothing abnormal in speech or deglutition. There is, however, a great increase of the paralysis of the arms and legs. Till to-day, he has been able to shuffle about the ward and use his knife and fork. To-day he is absolutely confined to bed, being unable to get up even for necessary purposes. The urine, after having ceased to be albuminous, or only showing occasionally a trace of albumen, had this morning a considerable albuminous deposit when treated by heat and nitric acid.

There is almost total cutaneous anæsthesia of the upper and lower extremities. Both legs are very much more paralyzed than at the date of his admission. There is less power in the left than in the right leg. He can voluntarily, but with much difficulty, draw up both legs to a certain extent. He can draw up the left less than the right leg. After he had drawn up his legs, at my request, five or six times, they were absolutely powerless. In about half an hour, the power began slowly to return to the legs. [I was informed next day that in two hours it was restored to the degree in which it existed before it had been exhausted, in the manner now described.]

The paralysis of the arms and hands is almost as great as that of the legs. Both deltoids are shrunken and wholly useless. The grasp of each hand is exceedingly feeble; and, after a few seconds, the feebly-grasped object falls from the hand. This was particularly observed when the nurse placed before the patient, on one of the hospital bed-tables, a plate containing a fork and some morsels of very tender meat. He managed, after two or three failures, to get the prongs of the fork into the meat; and after a rest of one or two minutes, he carried the fork towards his mouth, at the same time meeting the morsel, as it were, half-way, by bending down his body and head. Once or twice he managed to get the bit of meat into his mouth, but generally the fork fell from his grasp before he could get the morsel within his lips. Whilst the nurse fed him, I proceeded with the examination of a patient in an adjoining bed, so that the whole of the feeding, occupying probably half an hour, was conducted under my immediate inspection. When I asked the patient to squeeze my hand with all his strength, I could only feel the slightest possible pressure. At the same moment that I recognised this slight pressure, I saw the patient's fingers encircling my hand, and could distinctly perceive by his countenance, that, in obedience to my request, his volition to squeeze was at maximum intensity.

Finding that the bed was soaked with urine, which was dribbling from the penis, and finding by percussion that the bladder was enormously distended, I introduced a catheter, and thereby withdrew a large quantity of turbid dark-coloured urine, which, tested by heat and nitric acid, was found to be moderately albuminous. To prevent recurrence of over-distension of the bladder, which was not only the necessary consequence, but also the intensifying cause of the vesical paralysis, I have introduced a corked gum-elastic catheter and so attached it as to remain in position. The nurse is directed, at intervals of four hours, to let the urine flow from the bladder by removing the cork from the catheter. By keeping the catheter closed, except when the bladder is being evacuated at intervals, the risk of the organ contracting upon, and being injured by, the point of the instrument is obviated. On the other hand, the periodical evacuation at stated intervals is a security against over-distension. The cinchona and strychnia are suspended; the aperient dinner pill, containing one grain of the extract of nux vomica, is continued; and a tumbler of Püllna water is prescribed, to be taken every four hours till the bowels are freely moved.

6th March.—The Püllna water, aided by a clyster, thoroughly relieved the bowels at an early hour this morning. The patient looks better; but, in respect of the paralysis, his condition is the same, or worse. There is squinting in both eyes; and on placing before the patient a book printed in large, distinct type, he is surprised to find that he cannot read it. Unknown to himself, he has for some days been gradually losing the power of distinguishing small objects; even objects of considerable size he now sees indistinctly. For example, he cannot see whether the nurse has or has not a cap on her head. Besides the paralysis of the muscles which move and adjust the eye, he has palsy of the palpebral muscles.

On *7th* and *8th March*, the patient slept and ate well; and expressed himself as pleased and surprised at his great return of strength. On neither of these days did he take any medicines.

On *9th March*, he was in equally high spirits; he said that he had slept well, eaten a good breakfast with a good appetite, and felt in all respects much better. His tongue was clean; his pulse was 76, and of fair strength so long as he remained recumbent. He felt confident that he could stand and walk. I allowed him to try. With great difficulty, he got his legs over the side of the bed. On attempting to stand, he slid down on the floor, a helpless inert mass, and there remained till he was lifted into his bed. This unexpected display of muscular impotence caused him to weep. His condition was then carefully examined. He was found to have complete muscular paralysis and cutaneous anæsthesia of the upper and lower extremities. There was squinting of both eyes, and defective vision depending evidently on muscular paralysis. There was also palpebral paralysis. There seemed to be great decrease in the vesical paralysis. That the degree of this decrease might be accurately ascertained, I removed the catheter, and did not reintroduce it. I ordered a continuance of the full diet, including a daily litre of claret, which he had been having (and thriving upon) for some time. I directed that, till further orders, he should take no other medicine than the daily pill composed of one grain of extract of nux vomica, and three grains of the compound rhubarb pill of the British Pharmacopœia.

The therapeutic opportunity seemed now to have fully arrived. Although alimentation was being satisfactorily conducted, and a general corresponding functional amendment was obviously taking place, it was evident that nature's efforts were inadequate, and required to be judiciously assisted by art. There was, notwithstanding the conspicuous general improvement, a considerable progressive wasting of the paralyzed muscles. There was therefore a risk that if we did not soon excite movements of the affected muscles, their atrophy would proceed so far as either seriously to interfere with, or entirely prevent, a restoration of their powers. The indications were to promote improved nutrition of the wasting muscles by directing towards them a greater supply of blood, and by compelling them to involuntary, as a substitute for voluntary exercise.

I ordered gentle galvanic excitement of the nervous periphery of the superior and inferior extremities to be produced morning and afternoon to such an extent, and in such a way, as to induce moderate action of the paralyzed muscles. I likewise directed the whole circle of the muscles of the thoracic parietes and shoulders to be moderately kneaded twice in the twenty-four hours, the parts kneaded being at the same time well lubricated by a liniment composed of one part of tincture of cantharides, one part of British laudanum, and six parts of the British compound liniment of camphor. I likewise directed bands of linen, each one inch in breadth, and smeared with a stimulating paste, to be applied at intervals of five or six inches to the whole length of the superior and inferior extremities, care being taken to change the position of the bands once, twice, or oftener in the twenty-four hours, so that, while the surface was kept glowing with warmth by the paste, its topical action on the skin should not be allowed to exceed the proper limit. The composition of the paste employed in this case was not exactly the same as I usually prescribe in similar cases. The formula requires to be modified according to the character of the skin of the patient, the duration of the application, and other circumstances. In the case now being described, the paste was composed of six drachms of powdered ginger, and two drachms of English mustard, thoroughly rubbed up with just a sufficient quantity of lard to make a paste of suitable consistence.

On 11*th March*, the patient complained so much of continued pain in the limbs for hours after they had been galvanized, that the galvanization was ordered to be discontinued ; and as he attributed restlessness and sleeplessness on the two preceding nights to hot tingling sensations induced by the stimulating paste, I directed that the bands should be removed each evening at seven o'clock, and not replaced till the morning, when he awoke refreshed by sleep.

31*st March.*—The treatment prescribed on 9th March has been ever since steadily and rigidly carried out. Several slight vesications were on two or three occasions produced by the paste having remained too long in contact with the same surface. For weeks there has been no difficulty with the bowels. Tepid-water enemata, and a tumbler of Püllna water, have, however, been occasionally required to supplement the rhubarb and nux-vomica pill. In the legs there is a great increase of power, more sensibility, and less numbness. He moves his legs in bed without much difficulty. He is still unable to stand for more than half-a-minute ; and now (during the writing of this report), in attempting to do so, his legs gave way under him, and he slid down into a sitting posture. He is able slowly, and with occasional rests, to feed himself with spoon and fork, but he has not sufficient strength to use the knife to cut his food. His vision has much improved, and is daily improving. He has not the slightest difficulty in retaining or voiding his urine. The improvement has gone on so satisfactorily that it has been deemed unnecessary to recur to the use of galvanism.

The treatment is to be continued in all respects as at present ; and, in addition, he is ordered to take, three times a day, three grains of the *Br. Ph.* saccharine carbonate of iron.

30*th April.*—He is able, with the aid of a stick, to walk about the ward. His general health is good, and his appearance is that of a man in perfect health. With the exception of taking the saccharine carbonate of iron, as prescribed on 31st March, generous alimentation is now to be the only treatment.

On 10*th May*, the patient left the hospital in excellent health He walked with the aid of a walking-stick to the cab, which I saw him enter without the slightest assistance. On several recent occasions the urine has been examined for albumen without any being found.

On 1*st August*, he came by request to my house. He was then quite free from all trace of paralysis, in better health than he had enjoyed since the war; and was occupied with business involving a great deal of walking about the streets.

The case of S. M., very carefully observed, and now minutely described, affords in itself most of the elements necessary for a complete study of the paralysis of diphtheria; but the history of no single case can convey the whole truth regarding the natural course, pathology, and treatment of such affections.

CASE VI. *Pharyngeal Diphtheria in Florence; followed, after an apparently complete Recovery, by Paralytic Affections in Paris; Recovery.*

In the spring of 1873, I was consulted by A. B., a gentleman aged 30, under the following circumstances:—He came to my house from his hotel in a carriage, into which and out of which he told me he had got with great difficulty, arising, he said, from "rheumatism and weakness of both legs." He attributed his condition to cold and fatigue, ten days previously, on his journey from Florence to Paris. In this opinion he thought he was confirmed by having had some cough and sore-throat since his arrival in Paris, and at least a week before his limbs began to fail. He complained of dysphagia, almost amounting to an inability to swallow liquids, and of being unable in the morning to see well enough to read the newspaper. After a good dinner and some glasses of sherry, he said he could read print quite well. When he told me that he had just recovered from a very severe illness before he left Florence, I naturally and correctly concluded that the malady from which he had suffered in Florence was diphtheria, and that the paralytic symptoms which had supervened in Paris were ordinary sequelæ of the primary affection. He informed me that his diphtheritic attack in Florence had been exceedingly severe in respect to the prostration of strength, but that the pellicle had not extended, he believed, from the pharynx into the larnyx. The disease, in other words, had stopped short of "croup"—to adopt the phraseology of French physicians.

During the first few days under my care, the symptoms increased in severity. The patient was then taking cinchona and extract of nux vomica. The medication was discontinued, and in place of it Orezza water was prescribed. I directed it to be mixed with his wine at luncheon and dinner. He was unable to bear this strong ferruginous water, and discontinued its use after two days. Increasing dyspepsia now intervened, and seriously interfered with his alimentation. The paralytic symptoms continued, but did not acquire greater intensity: in some respects they were less intense. He was falling into a very depressed state of mind and body, when, about ten or twelve days after he came to me, I ordered him to take before his two principal meals a pill containing three grains of pepsina porci, one grain of saccharine carbonate of iron, and one grain of salicine. Under the use of these pills and a generous diet of varied meats, vegetables, and wines, he rapidly improved, and entirely recovered within four weeks from the day on which he first consulted me. He soon afterwards left Paris, since which time I have not heard of him.

I twice examined the urine for albumen without finding any trace of it. It is quite possible, however, that some might have been found had my examinations been more frequent; for albuminuria is more commonly intermittent than persistent in diphtheria and diphtheritic paralysis.

The cases of E. G., S. M., and A. B., when viewed together, show that there is no relation between the severity of the primary symptoms and the subsequent paralytic affections.

The six cases which have been described, illustrate the most important points in the diagnosis, prognosis, and treatment of Diphtheria, and are suggestive of the signification of albuminuria in that disease.

DIAGNOSIS.

The invasion of diphtheria is generally occult and insidious. The disease takes full possession of its victim, in the majority of cases without exciting the least alarm in him or in those by whom he is surrounded. So it happened in the cases of E. G. and Miss P.

When E. G. was at home from school, on Sunday the 19th October, his family thought that he was not quite well, though not so ill as to require any special attention. The patient was at that date already under the influence of the nascent disease. On Monday and Tuesday, 20th and 21st October, the schoolmaster imputed to childish tricks and idleness, conduct which was really the consequence of advancing illness. On the afternoon of Wednesday the 22d, the child, being broken down with diarrhœa, was sent home as an invalid. On Thursday the 23d he complained for the first time of sore-throat, and was affected by dyspnœa and aphonia. These symptoms, if observed by an experienced medical practitioner, would no doubt have led him to suspect, and probably to discover, the existence of diphtheria. They did not, however, alarm the family, by whom they were not unnaturally attributed to catarrh caused by the child's having been sent home imperfectly clad when weakened by purging. Early in the morning of Friday the 24th, when I first saw the patient, the progress of the case had removed all difficulty from the diagnosis. The history of the case, and the general character of all the symptoms, pretty clearly showed the nature of the malady; and there was one sign which established the diagnosis beyond the possibility of error—the scrap of tough membranous pellicle which I stripped off with the forceps from the surface of one of the tonsils.

The insidiousness of the invasion of diphtheria is also very strikingly illustrated by the history of the case of Miss P. When I first saw that patient, the diagnosis was easy for the medical, but not for the general observer, who would even have been unable to realize the existence of any serious disease, so slight and ordinary were the symptoms in the eye of inexperience. Though I have carefully traced back Miss P.'s history into the routine of ordinary health, I feel that it is impossible to be certain whether the "cold in the head," from which she was suffering on the 24th December, was the first part of the train of symptoms which ran on continuously from the 29th December till death on the 3d January. At my first visit to the patient on the 31st December—four days before her death—I found her reclining in apparent comfort on a couch, and I was told that she was suffering from "sore-throat" and "biliousness." In point of fact, there was nothing in her state to produce disquiet in the mind of inexperienced observers.

The invasion-symptoms of diphtheria are nearly always slight as well as insidious—a circumstance which imparts great difficulty to the diagnosis at an early stage of the disease. Families do not take alarm at the slight hoarseness of a child who is amused with his toys. Slight fever, a little languor, an occasional tickling cough, and a loss of voice, are all symptoms which, both individually and in a group, are of frequent occurrence in children. They may be only transient catarrhal symptoms; but for the first three or even four days a diphtheria-stricken child will probably present no other indications of abnormal health. It is therefore a paramount duty of the practitioner when called to a child with what is apparently only a slight sore-throat, to make an examination of the throat with great care, internally and externally, and to weigh what he sees together with all the information he can obtain regarding the history of the case and the general symptoms. This scrutiny will be imperative even with adults, if diphtheria be prevailing as an epidemic, or if the patient be living in contact with sporadic cases, or if he have been in any way exposed to the contagion of the disease. However skilful and minute the scrutiny of the medical observer, he will probably be unable, for some days after the commencent of the disease, to establish the diagnosis on a positive basis. In the invasion-stage it is seldom possible to form an absolute diagnosis. The early symptoms being generally slight, and common to diphtheria and to trivial maladies, the disease is often allowed to pass into the exudation-stage before medical aid is sought. In point of fact, there is nothing uniform or characteristic in the symptoms of invasion. General discomfort, loss of appetite, debility, diarrhœa, rigors, pallor, husky voice, and hoarseness very often usher in diphtheria; but as they often also usher in various other maladies, they assist our diagnosis only when viewed in conjunction with other circumstances.

Invasion-symptoms may be absent, or the first symptoms which attract attention may be misleading. The invasion-symptoms, as well as the whole course of the cases, vary with the epidemic. This fact has been lost sight of by some systematic writers, who have artistically combined materials which, for the sake of clinical truth, ought to have been represented in separate pictures.

The poison of diphtheria may, it would appear, remain latent in the system for a long period—for how long, it is impossible to say—and then, upon occasion given, suddenly manifest itself. In this respect, diphtheria may perhaps resemble ague, particularly some of the forms of ague to which the designation "pernicious" has been applied.

There is a class of facts which tends to prove that the poison of diphtheria may remain latent in the system till circumstances occur to call it forth. The seed, so to speak, is received into unsuitable soil, where it may either perish after a time, or remain in a state of latent vitality till the production of some change in the soil—or, in other words, in the condition of the patient—excites the reproductive power of the morbid germ. "You will see," says Trousseau, "in children who have been blistered on the arms for catarrhal affections, a very common practice, and which may even have been resorted to by medical practitioners—you will see the blistered surfaces become covered with false membrane, *if the children are living in the midst of diphtheritic contagion*."[1]

I have seen a catarrhal affection of the throat, the result of exposure to cold, become the starting-point of pharyngo-laryngo-tracheal diphtheria. Such cases have often been described as "catarrhal diphtheria," as if they constituted a variety of diphtheria; but it is not so, for the catarrh does not come from the diphtheria: on the contrary, the diphtheria comes, in a sense, from the catarrh, the latter being a lesion which acts as the immediate determining cause of the diphtheritic manifestation, though neither the cause, nor a part of the diphtheria.

The fact that persons "living in the midst of diphtheritic contagion"—to use Trousseau's expression—may really be dangerously charged with the poison, even when apparently uncontaminated by it, is very suggestive of the importance of adopting prophylactic treatment in districts, houses, and families.

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[1] Trousseau, Lectures on Clinical Medicine. See p. 588 of second vol. of New Sydenham Society's Translation. London, 1869.

The diagnosis of diphtheria is frequently surrounded with great difficulties after the disease has passed out of the invasion-stage, and even when it has reached the period of cacoplastic exudation.

What are these difficulties, and how can we obviate them? What are the diagnostic signs of diphtheria? It is only the experienced practitioner who can fully understand and successfully grapple with the difficulties of these questions. The diagnostic rules laid down in systematic treatises require to be supplemented and corrected by that kind of knowledge which can only be obtained by clinical study—that is to say, by a minute and patient study of cases at the bedside.

As a general rule—with, however, one or two per cent. of exceptions—the diphtheritic exudation first manifests itself on the tonsils or pharynx, and next—not always in continuity—on the larynx. From one to seven days may elapse between the first appearance of the false membrane and its exudation on the mucous surface of the larynx. In some epidemics, the larynx is a usual situation for false membrane to be first formed. In more than one sporadic case, I have seen the first deposit of false membrane occur in the bronchial tubes, then in the trachea, and death take place without the larynx having been invaded. Many such cases are recorded by reliable observers, who have compared the symptoms and phenomena during life with the autopsic appearances. The first case detailed in this paper—the case of the child taken to the ambulance—was of this kind. As I have already said, it was a case to which the term "bronchial diphtheria" might be applied to indicate the starting-point of the diphtheritic exudation.

Confluent herpes of the throat has often been mistaken for diphtheritic sore-throat.

In October 1875, I was in attendance on a lady about fifty years of age, who for nine days suffered very severely from a confluent eruption on the roof of the palate, tonsils, and back part of the pharynx, accompanied by swollen submaxillary and cervical glands. Together with these local affections, she had pains in the back and limbs, hoarseness, with occasional aphonia, anorexia, frequent vomiting, acrid eructations, bitter taste in the mouth, loathing of food, incessant cough (which sometimes occurred in suffocative paroxysms), sneezing, and coryza. This case, by mimicking certain phases of diphtheria, gave me some anxiety for forty-eight hours; but at the end of that period, and before the severity of suffering had moderated, an absolute diagnosis was established, and a favourable prognosis formed, in consequence of a fresh herpetic eruption appearing on the lips and roof of the mouth. The duration of illness in this case of confluent herpes of the throat was nine days. The treatment consisted in the use of mouth-washes, and in administering bismuth, antacids, vegetable tonics, pepsine, and mild laxatives.

The successful treatment of diphtheria, so much boasted of in the recent writings of some practitioners, may probably be explained by their having had to do with the essentially different disease—throat-herpes, called *herpes guttural* by Gubler, who was the first to give a good description of it. Cases occur in which the differential diagnosis of diphtheria of the throat, and confluent herpes of the throat, is not without a certain amount of difficulty. Whenever this difficulty occurs, the treatment ought to be such as may do good, and can at least do no harm, in diphtheria.

Trousseau states that it is sometimes impossible to determine whether a sore-throat be diphtheritic or herpetic. He says:—"The embarrassment is still greater, both in adults and in children, when, as often occurs, the characters which distinguish diphtheritic from herpetic membranous sore-throat are not unmistakably clear. As Bretonneau has justly remarked, the question can sometimes only be solved by the dangerous tendency of the diphtheritic affection to extend to the tonsils, pharynx, and respiratory passages. During an epidemic, when the diagnosis is undecided, we ought in every case to be as prompt to act as if we had real diphtheria to combat; for it is better to treat energetically a malady which is not serious, than to run the risk of allowing one of an essentially malignant character to gain ground."[1]

In many cases, as in that which I have just described, the appearance of herpes on the lips removes all difficulty from the diagnosis. The manner of invasion in the two diseases is generally quite different. An attack of diphtheria of the throat is insidious; an attack of herpes of the throat is ushered in by an acrid burning in the throat, fever, and disorder of the stomach. The best help, however, in cases of difficult differential diagnosis between diphtheritic sore-throat and herpetic sore-throat, is based on the characteristic feature of diphtheria, viz., a specific exudation from mucous surfaces, wounds, and cutaneous abrasions. The diagnosis between diphtheritic croup, and the various kinds of non-diphtheritic membranous sore-throat, can frequently be established without any harm to the patient by applying a *very small* blister to a convenient part of the body, such as the forearm. If the disease be diphtheria, the diphtheritic pellicle will probably appear on the vesicated surface.

In mild cases of pharyngeal diphtheria, the mildness of the disease sometimes causes its nature to be unsuspected, and the patient recovers from what was looked on as a common sore-throat. Even, however, in the mildest cases of simple pharyngeal diphtheria, the appearance of the false membrane on one or both tonsils will be sufficiently characteristic to establish the nature of the case.

In some localities, and in some epidemics, diphtheria is much milder than in others. The general poisoning is more moderate, and the formation of false membranes does not proceed beyond the pharynx. The majority of such cases recover without treatment, and whether the treatment be good, bad, or indifferent. Some practitioners who have seen diphtheria only or chiefly in this mild form have complacently attributed the mildness of their cases to their own skill, and have expressed surprise that their methods of treatment were not universally adopted as soon as they were made known.

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[1] Trousseau, Lecture on "Membranous Sore-Throat, and in particular Herpes of the Pharynx." See page 439 of vol. ii. of New Sydenham Society's Translation of the Clinique Médicale. London, 1869.

In *scarlatina*, the tonsils, pharynx, and nares often present appearances considerably resembling those which belong to diphtheria. This remark is specially applicable to the gangrenous ulceration of the tonsils met with in malignant forms of scarlatina. There is often in malignant scarlatina a membranous-looking coat on the tonsils, sometimes mistaken for the false membrane of diphtheria, to which it bears a general resemblance. If, however, the history of the patient's illness, the consistence of the membrane, and its situation, be studied with a view to the induction, a correct conclusion will generally be formed by a careful and experienced observer.

Speaking of the diphtheritic sore-throat often engrafted on scarlatinous sore-throat, Trousseau says:—"The patients certainly sink with all the symptoms of diphtheritic poisoning, such as lowering of the general temperature, a small pulse, and fœtor of the breath exhaling from the mouth and nose, and a general paleness of the skin—a combination of symptoms not met with in any other serious disease. We can suppose that in persons placed under certain conditions—as, for example, in a centre of epidemic diphtheritic influence, such as is, one may say, always dominant in hospitals for children—the scarlatinous sore-throat may become the starting-point of a diphtheritic attack, exactly in the same way as a small excoriation behind the ear, an ulceration of the vulva, or any other solution of continuity existing in persons in the midst of erysipelatous epidemic influences, may become the starting-point of erysipelatous manifestations. A circumstance which tends to support me in looking at the facts from this point of view is this, that I can only recollect one case of recovery from sore-throat supervening suddenly at the ninth or tenth day of an attack of scarlet fever."[1]

In the passage now quoted, Trousseau does not refer to malignant scarlatinous sore-throat, an affection in which a pultaceous deposit is generally formed.

In the same lecture he makes some excellent remarks on the differential diagnosis of the sore-throat of diphtheria, scarlatina, measles, and smallpox. He says:—"True scarlatinous sore-throat is pharyngeal, differing in this respect from the sore-throat of measles, which is laryngeal, and from that of smallpox, which is both pharyngeal and laryngeal. The voice of scarlatinous patients, when affected, is snuffling, but its tone is sonorous; the voice does not undergo the modifications to which it is subjected in the other form of sore-throat when it is traversing the throat, nose, and mouth. In measles it often happens that the tone of the voice, very much altered during its formation in the larynx, undergoes no farther change in traversing the back part of the throat."

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[1] Op. cit., vol. ii. p. 180.

Certain forms of sore-throat accompanying the exanthemata are more or less liable to be mistaken for the sore-throat of diphtheria. The medical history of each case will generally be sufficient in itself to establish the diagnosis. In respect of scarlatina, however, we must remember that at its subsidence a sore-throat may set in which is not scarlatinous, but is a real membranous, that is a diphtheritic, sore-throat—a manifestation of the general disease diphtheria, to which convalescents from scarlatina are peculiarly disposed in certain localities and during epidemics of diphtheria.

Cutaneous eruptions, some of which have a great resemblance to the eruption of scarlatina, frequently make their appearance in the course of diphtheria. In the case of E. G., I have stated that on the 26th October—probably the seventh or eighth day of the disease—a bright rubeolar eruption was conspicuous on the trunk and on the legs. Sometimes, if the eruption were the only guide to the diagnosis, it might easily be declared to be one of the forms of the eruption of scarlatina. The eruptions of diphtheria which simulate those of scarlatina are sometimes vesicular, sometimes like urticaria, and sometimes they occur in large bright red patches. Unlike the eruptions of scarlatina, they are not followed by desquamation. That they are not scarlatinous is sometimes proved by their being succeeded by the true scarlatinous eruption; for convalescents from diphtheria are as much disposed to take scarlatina as are convalescents from scarlatina to take diphtheria.

The eruptions of diphtheria seldom continue more than three days, and sometimes are visible for only a few hours. They differ much in appearance; yet we must nevertheless look upon them as the results of the special contagium of diphtheria, just as the different though less varied eruptions of scarlatina proceed from the special poison which engenders that malady, or as the manifold eruptions arising from toxæmia are dependent on morbid and non-morbid poisons.

The *general symptoms* of diphtheria may, for descriptive purposes, be divided into two classes, viz., the *constant* and the *occasional*, though, as will be seen by the sequel, these terms are not strictly correct.

The *constant* general symptoms are—

Prostration of strength; and
Formation of false membrane.

The *occasional* general symptoms of most importance and frequency are—

Albuminuria;
Enlarged glands; and
Paralytic affections.

Prostration of strength is a leading symptom in all forms of diphtheria. In some cases, at the very onset of the attack, it is so overwhelming as to destroy the patient before the ordinary phenomena of diphtheria have had time to show themselves.

False membrane is *almost never wanting in diphtheria,* when the patient lives long enough to allow time for its appearance. The word *almost* is required to qualify the general statement, for in diphtheria, as in all other poison-diseases, exceptional abortive cases occur—cases in which, from arrest of development of the morbid germ, there is not time for the disease to run its natural course. One kind of arrested development consists in the non-appearance of the pellicle, as my friend Michel Peter has well stated in his inauguration thesis, where he uses as nearly justifiable terms "diphthérite *sine diphtheria,*" "rougeole *sine morbillis,*" and "scarlatine *sine scarlatina,*" all of which arrestations I have seen during epidemics of the respective diseases, and also in sporadic cases.

PROGNOSIS.

The prognosis in diphtheria is always doubtful, except in cases which, from the first, exhibit obvious symptoms of danger. As a rule, in the early stage of the disease, even when the attack is apparently very mild, the greatest reserve must be exercised in forecasting the issue. The general and local symptoms of ultimately fatal cases may for some days have presented nothing to indicate a great deterioration of health, when suddenly the specific exudation, till then limited to a small surface on the tonsils or pharynx, manifests itself in the larynx, rapidly proceeding it may be to the occupation of the trachea and the larger and smaller bronchial tubes; or the mild general symptoms may all at once become serious and abruptly terminate in prostration and death, without any great extension of the diphtheritic pellicle; or, again, in cases seemingly mild at first, suffocation may terminate life, the rapid formation of false membrane in the larynx mechanically occluding the air-passage, or partly by mechanically preventing the entrance of air, and partly by exciting spasmodic constriction of the glottis. The asphyxia which so nearly extinguished life in the case of E. G. was partly due to spasm, and partly to membranous obstruction; but it was the membranous obstruction alone which justified tracheotomy.

The prognosis can only be provisional in most cases—that is to say, it must, if favourable, be made with reserve, to the effect that a sudden change for the worse in the character of the symptoms is not unusual. The character of a prevailing epidemic will often be a surer guide to correct prognosis than the symptoms of the individual case. In some epidemics the prostration is moderate, and amenable to average skill in alimentation and medicinal treatment, while the local manifestations of the disease are limited to the pharynx. In some of the milder epidemics of this character—that is to say, epidemics of simple pharyngeal diphtheria—nearly all the patients recover, and that too with very little or without any medical assistance; or it may be, perhaps, notwithstanding the most irrational and rash interference with the progress of the natural cure.

When the larynx and trachea become invaded with false membrane—that is to say, when the case becomes laryngo-tracheal diphtheria—death is the most common termination; but here also the character of the prevailing epidemic must be taken into account. In sporadic cases, and in most epidemics, the statement now made is correct—death is the rule, and recovery the exception. In the French epidemic of 1847, the mortality was computed as 91 in 100 cases. Age exerts a remarkable influence on the mortality—the younger the subject the greater the risk. This probably arises from two causes: the less power of resisting the morbid poison at an early age, and the narrowness of the larynx in young children. The narrower the larynx, the greater is the danger of its occlusion.

The basis of a prognosis in a case of diphtheria is a knowledge of the duration of the attack at the time of forming the prognosis. The prognostic signification of symptoms in respect to their nature, grouping, and severity, can only be determined after ascertaining the age which the disease has attained. Diphtheria, like all other poison-diseases, has, it must be remembered, a distinctive natural course to run. This is not the less true, because it frequently happens that disturbing circumstances occasion divergences and abrupt arrests before all the posts have been passed.

If the patient at the eighth day of the disease be able to take and assimilate a fair amount of aliment—if also, at the same period, no exudation of cacoplastic lymph into the larynx, trachea, or bronchial tubes have taken place, the false membrane has been ejected and is not being reproduced—the prognosis is hopeful, even although the urine be albuminous, and the action of the heart very feeble. By maintenance of the horizontal position, the judicious use of stimulants, careful alimentation, and the assistance of digestion by pepsine, recovery is likely to take place. Circumstances may likewise render it advisable to add to this treatment the administration of ferruginous and other medicines; but unless the horizonal position be steadily enforced, and the alimentation be properly carried out, a hopeful prognosis would not be justified by the condition now sketched. Persons in that condition often die suddenly through uncautioned or incautious attendants allowing them to leave their bed, or sit up in bed, to satisfy the calls of nature, when the heart's power is only sufficient to carry on the circulation so long as the patient remains recumbent. In point of fact, the reliability of the nurse is a much more important element in the prognosis of diphtheria than in that of most diseases, because in most other diseases death is less apt to occur suddenly from loss of heart-power.

The worst prognostics in diphtheria are incoercible vomiting, extreme prostration occurring early, hæmorrhages, pneumonia, broncho-pneumonia, and the formation of false membrane on the surface of the larynx, trachea, and pulmonary air-passages.

Albuminuria is not an unfavourable symptom, except as an expression of defective assimilation. A trace of albumen is certainly not an unfavourable sign; and recoveries frequently take place—as in that of E. G.—in which the urine has been intensely albuminous.

Epistaxis is generally the precursor of diphtheritic exudation into the nares.

There are few cases of diphtheria, however bad, which the physician is justified in declaring irrecoverable, till death has actually taken place. By tracheotomy, by the use of stimulants, or by both measures combined, time is gained, and therapeutic opportunities arise, by skilfully using which patients are frequently snatched from the very jaws of death.

The best prognostics in diphtheria are cessation of exudation, increasing heart-power, and improving assimilatory function.

PROGNOSTIC SIGNIFICATION OF ALBUMINURIA.

The observations on the urine in the cases detailed in this paper are few and incomplete, but they nevertheless possess considerable value when viewed in connexion with the reports of the other symptoms.

In Case I. there was no opportunity of examining the urine.

In Case II. (Miss P.) the urine was not albuminous. The patient, who died after a very short illness, took food freely till within an hour of her death.

In Case III. the urine was not examined. I only saw this case occasionally in consultation.

In Case IV. (E. G) the urine was often intensely albuminous. The albuminuria was, however, variable in degree at very short intervals, and had an evident relation to the time of feeding and the quantity of food taken.

In Case V. (S. M.) the urine was at intervals albuminous during the course of the paralytic affections. It is noteworthy that on the 5th March constipation, increase of paralysis, and increase of albuminuria were coincident. At that date, I believe the patient had been overfed.

In Case VI. (A. B.) the patient had pharyngeal diphtheria in Florence, followed, after an apparently complete recovery, by paralytic affections in Paris. I twice examined the patient's urine when he was under my care for the paralysis, and on neither occasion found any trace of albumen.

The prognostic signification of albuminuria in diphtheria has been very differently estimated by clinicians of repute. For example:—

Trousseau says:—"The presence of albumen in the urine of diphtheritic patients . . . is a frequent occurrence, but one which in the actual state of our knowledge has only a limited signification in relation to prognosis and treatment. It is, however, impossible to deny that it is the expression of a great disturbance of the organism, produced by the morbific principle which engenders diphtheria."

Jenner says:—"Even a trace of albumen in the urine is an unfavourable symptom; when very abundant, a fatal termination of the case is most probable."

Morelli and Nesti, in their admirably-told clinical history of diphtheria at Florence and in its vicinity, from 1862 to 1872, say:—"In the morbid constitution which has been here dominant for the last two years, and which still prevails, we have had opportunities of noting the coexistence of albuminuria and diphtheria, having found albumen in the urine frequently and in large quantity in the present epidemic. Transitory and intermittent albuminuria constitute at present a morbid phenomenon of very frequent occurrence in cases of diphtheria. It has not, however, appeared to bear that constant relation to the course and the issue of the disease attributed to it by Bouchut, particularly when affecting the respiratory passages, to the effect that its appearance prognosticates an unfavourable, and its disappearance a favourable, termination. Among the many cases of simple diphtheria which I have seen, albuminuria does not hold that important relation, and whilst it is absent in cases of very great severity, it is detected in the course of very mild cases."

The conclusion which I have arrived at, from the careful observation of many cases, has been already expressed, to the effect that *albuminuria is not an unfavourable symptom except as an expression of defective assimilation.* Emaciation and albuminuria are both very frequent in diphtheria: they often coexist, and both are consequences of one cause. It is in that light that I look upon the existence or absence of albuminuria in diphtheria as significant in respect to prognosis and treatment. In my experience a trace of albumen in the urine has not proved an unfavourable symptom, nor has intensely albuminous urine by itself seemed to foretell a fatal termination.

As an expression of defective assimilation it is an unfavourable sign, I admit. But if the albuminuria be intermittent, as it generally is, the degree in which it is unfavourable is less. It is then significant of the necessity to modify the dietetic system which is being adopted.

Dr W. F. Wade, of Birmingham, made known to the medical profession the occurrence of albuminuria in diphtheria by a paper which he published in the *Midland Quarterly Journal of the Medical Sciences* for April 1858. This paper was soon afterwards quoted in the *Archives Générales de Médecine*, and was thus brought under the notice of Trousseau, who in his published lectures gives Dr W. F. Wade the credit, justly belonging to him, of having first brought the subject of albuminous urine in diphtheria under the notice of the profession at large. Dr W. F. Wade first announced his discovery on the 15th December 1857. On that day, at a meeting of the Queen's College Medico-Chirurgical Society of Birmingham, he showed kidneys removed from the body of a patient who had died of diphtheria. Upon that occasion, he called the attention of the Society to the morbid changes presented by these organs, and asked the members present to examine the urine of their diphtheritic patients, feeling assured, as he then stated, that the urine was albuminous in the subject from whom the exhibited kidneys had been taken. A physician present examined the urine of a diphtheritic patient on the following day, and soon afterwards he and others announced that they had, as a rule, discovered albumen in the urine of diphtheritic patients.

Trousseau, while he awards the palm of priority to Dr W. F. Wade, intimates that before he became acquainted with that physician's researches, he knew that the urine is often albuminous in diphtheria, having learned that fact from an unpublished memoir of Abeille. Trousseau, referring to Dr W. F. Wade's discovery, says :—

"This discovery, from having been originally published in the *Midland Journal of the Medical Sciences*—a periodical little circulated on this side the Straits of Dover—remained for a long time unknown in France. Like everybody else, I was ignorant of the discovery, when there fell into my hands an unpublished paper by Dr Abeille, who was the first, to my knowledge, to mention diphtheria among the diseases in which we meet with albuminuria. Since that time, I have lost no opportunity of looking for albumen, which I have several times found in the urine of diphtheritic patients in the clinical wards, and did not fail to notice in my clinical lectures during 1857. In a lecture delivered on the 23d June 1858, Dr Germain Sée, ignorant of the researches of the English physician and of Dr Abeille, called, in a more particular manner, general attention to the frequency of albuminuria occurring in malignant sore-throat and in croup, both before and after tracheotomy. He stated, that in his wards in the Children's Hospital, the urine of all the diphtheritic patients was examined for albumen every day, and that at least in one third of the cases it was found in notable quantity. It is, therefore, as Dr W. F. Wade originally stated, and as I have verified before you, very common to find albumen in the urine of diphtheritic patients."[1]

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[1] Trousseau, Lectures on Clinical Medicine, vol. ii. p. 537 of New Sydenham Society's Translation. London, 1869.

In 1858, the same year in which Dr W. F. Wade's paper appeared, Dr Germain Sée brought the subject of albuminous urine in diphtheria under the notice of the Société Médicale des Hôpitaux of Paris. Since that date, it has been treated of by numerous British and foreign authors, some of whom have written from clinical data occurring under their own observation. Dr W. F. Wade was first in publication; but it is equally certain that Dr Abeille, Dr Germain Sée, and Dr W. F. Wade made the discovery independently of each other, and at about the same date.

Individual peculiarity and the different constitution of epidemics greatly influence the presence or absence of this symptom, and also the degree of its intensity when present. Neither by degrees of albuminuria, nor by its presence or absence, can we gauge the gravity of cases. As in scarlatina, so likewise in diphtheria, I have noticed its absence in cases of maximum severity, and its presence in very mild cases. It varies much in intensity from day to day, and is not unfrequently intermittent. The difference in the prognostic importance attached to albuminuria by clinical authors of the highest class is probably explained by some of them drawing general conclusions from the limited number of cases they have had an opportunity of personally subjecting to rigid scrutiny. Diphtheria, except when epidemic, is not a common disease; and as it generally sets in very insidiously—very often unobserved—and runs a rapid course, comparatively few cases are minutely observed by skilled observers from their beginning to their end.

Albuminuria exists at a very early stage in many cases of diphtheria—often within the first twenty-four hours. In such cases it cannot be a secondary toxic phenomenon. The renal congestion found after death, and the albuminuria during life, are both direct consequences of a great blood-lesion—imperfect hæmatosis—resulting from a profound toxic impression on the hæmapoietic organs, produced through the nervous system by the diphtheritic contagium. Reliable authorities have observed and published cases in which the albuminuria was found to exist at so very early a stage of the disease as to preclude the supposition that it depended upon secondary toxæmia.

In the profound disturbance of the nervous system attending the death-agony, the urine, as Gubler and others have stated, is almost always albuminous irrespectively of the nature of the malady of the dying subject.[1] This observation, which I have repeatedly verified in hospital practice, has an interesting bearing on the subject now under consideration. In diphtheria and other poison-diseases, there is frequently from the very first a profound disturbance of innervation, to which, as in the death-agony, is chiefly due the imperfect hæmatosis and the consequent albuminous urine at an early stage of these maladies. When, however, the disease has gone on for some time, secondary causes may either originate albuminuria or intensify it, if previously existing.

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[1] Gubler, article Albuminurie, Dictionnaire Encyclopédique des Sciences Médicales, par Raigé. Delorme et A. Dechambre, pp. 482-483, tome ii. Paris, 1865.

In the semi-suffocative asphyxia of laryngeal diphtheria, the mechanically produced embarrassment of respiration and circulation will sometimes, no doubt, cause or increase albuminuria.

The albuminuria of diphtheria, as is well known, is often intermittent. Even when albumen is persistently present in the urine, its amount varies much in most cases during each twenty-four hours. This variation I have repeatedly observed; but, unfortunately, the observations were not made in a sufficiently methodical manner, nor recorded with sufficient exactitude, to form a basis of speculation, far less of induction as to the cause or causes. This I the more regret, as the cause or causes are probably ascertainable by subjecting a few cases to strict observation, and by minutely recording at the time the observations made. The urine would have to be examined each time it was voided, a record being made in respect to time of being voided, specific gravity, nature and quantity of the solid constituents, and the amount of albumen, and also of the exact time at which food and medicines were given, the kind and quantity of each being noticed. Except in hospital practice, with organized assistance at command, it is impossible to conduct clinical inquiries of this description; and since my attention has been directed to the cause of the intermittence and variations of albuminuria, the cases of diphtheria which I have seen have all occurred in private practice, during periods when imperative work occupied my whole time.

On reading the new and valuable clinical information contained in Warburton Begbie's paper on "Albuminuria in Vascular Bronchocele and Exophthalmos,"[1] it occurred to me that there may exist a very considerable similarity both in the essential cause of the albuminuria and in the cause of its intermittence in diphtheria, and in the cases described by the Edinburgh physician. In both diseases, the discharge of albumen with the urine may be due to the existence of two collateral causes, viz., the disturbed state of innervation, and the altered condition of the blood; and the cause of the sudden variations in the amount of albumen—the remittence or intermittence of the albuminuria—is probably in both diseases the passage into the blood of imperfectly digested ingesta. The imperfect digestion in exophthalmos is often dependent on bulimia, so common a feature in the disease; and in the case recorded by Warburton Begbie, this symptom was notably present. Though there exists usually in diphtheria a condition the very opposite of bulimia—a loathing of food in place of an inordinate craving for it—the assimilative power at a minimum, as is often evidenced by lienteric diarrhœa, and always by rapid emaciation, however much nutriment may be taken. It is easy to

[1] Edinburgh Medical Journal for April 1874.

understand that variations in the strain put upon the organs of assimilation by the ingestion and digestion of food may cause sudden and transient variations in the time and quantity of the albumen discharged with the urine by a diphtheritic patient.

The following remarks of Warburton Begbie on the albuminuria of exophthalmos are deserving of study, in relation to the albuminuria of diphtheria:—

"The form," he says, "of albuminuria which we are now considering differs from any other form hitherto described, in being limited to the period of digestion of the food. The presence of disease apart from the kidneys is of course conspicuous in vascular bronchocele and exophthalmos, while the peculiar morbid condition of the nervous system and of the bloodvessels in that disease, as well as the spanæmia which exists, must, I think, be taken into consideration in our endeavour to determine the pathology of the albuminuria, which we now know to be in some way or other associated with it.

"In vascular bronchocele and exophthalmos, there is always present much disturbance of the nervous system—the sufferers from this disease are invariably highly nervous—they are often hysterical. The primary disorder of the circulation, both cardiac and vascular, is of the nature which we associate with derangement of the nervous system. The organs and parts of the body in which the local manifestations of disturbance are seated, are organs and parts freely supplied with bloodvessels and blood—the thyroid gland, the spleen, which, although not invariably, is often affected, and the deep ocular tissues. To these must be added the kidney. From the failure of due nervous influence, the small vessels, and, it may be presumed, the capillaries, in the thyroid gland, and the deep-seated orbital vessels, become dilated, and the circulation through them in consequence interfered with. We can infer from the consideration of the essential nature of the renal circulation that if an obstruction to the return of blood through the intertubular capillaries and veins exists, either from an obstruction in the heart or lungs, or from a disordered state of the vessels themselves—a condition which I believe to exist in vascular bronchocele and exophthalmos—favoured by the more or less watery state of the blood itself, there will occur a transudation of serum, carrying with it albumen, through the walls of the Malpighian capillaries into the tubes, and thus the urine will be rendered coagulable."[1]

But in order to explain the limitation of the albuminuria to the period during and after digestion of the food, it is necessary to regard the increased afflux of blood which then takes place as leading to an altered physical relation between the blood and the walls of the vessels, and likewise determining an engorgement of the Malpighian capillaries, while the loss in tonicity in the different vessels is thus rendered temporarily more injurious. In other words, the renal circulation, in its comparatively tranquil condition, is unaffected by the disordered state of the capillaries and small vessels; but when excited by the stimulus of a recent meal it is unequal to the task, and the resulting interference determines the albuminuria.

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[1] *Loc. cit.*, p. 889.

The determining cause of albuminuria in a large proportion of the cases of diphtheria—its remittency and intermittency in many cases—and also the fact of its not being in itself an unfavourable prognostic in that disease—seem to be all explained by looking at the phenomenon from somewhat the same point of view from which Warburton Begbie regarded the frequent occurrence of albuminuria in vascular bronchocele and exophthalmos.

(To be continued.)

ARTICLE IV.—*The Structure of a True Mucous Polypus of the Cervix.*

By Dr C. E. UNDERHILL.

(Read before the Obstetrical Society of Edinburgh, 22d March.)

THIS tumour was removed from Mrs N., æt. 64, by Dr Matthews Duncan. The patient, who had ceased to menstruate for many years, began one and a half years ago to suffer from floodings which recurred at irregular intervals; they were sometimes attended by severe pain in the lower part of the abdomen and loins. On examination of the vagina, there was found, about two inches from its orifice, a flat elevated surface about as big as a shilling, which was smooth to the touch, but had the appearance of being corrugated and superficially ulcerated when seen through the speculum. Hanging from the os uteri were several small mucous polypi, too small to be removed as such, except one which was about the size of a horse-bean; they were cut away with the scissors, and the large one was hardened in absolute alcohol and examined; it measured half an inch in length, and about the same in circumference. It was of moderately dense consistence, and some tenacious mucus adhered pretty firmly to it. The operation was attended by little or no hæmorrhage.

On making a section of the little tumour, nearly one-third of the area of the sections was occupied by a cavity containing mucus; this was the only cavity visible to the naked eye at first, but on dividing it through the middle three or four smaller cavities came into view. Under a low power of the microscope, the uniform appearance of the surface was strikingly broken by a number of irregularly-shaped spaces, some large, some small, which were lined by a transparent uncoloured epithelium of large size; at the base of the transparent epithelial layer, and separating it from the general structure, was a narrower layer of tissue, which was much more deeply stained with carmine than any other part of the section. This layer alone surrounded the large cavity first mentioned, and had there no layer of transparent epithelium on its surface. The external surface of the section presented a somewhat papillary character, the papillæ being covered with cells of larger size than make up the bulk of the tissue; indeed, they can be differentiated as separate cells, with a power of about 80 diameters. Numerous bloodvessels stuffed with a brown mass can be seen lying most plentifully near the junction of the papillary epithelial covering with the rest of the tissue; they are not, however, confined to this part.

Under a higher power, 300 diameters, the stroma of the tumour is seen to be composed of connective tissue, with great numbers of very small round and fusiform cells in its meshes; a few bundles of fibrous tissue are interspersed here and there, especially in the neighbourhood of the glandular spaces. These spaces are now seen to be lined with a thick layer of long narrow transparent cells, having an oblong appearance in profile, being, however, rather broader at the base than at the apex, and exhibiting a very perfect regularity in shape and size. These cells can be seen in some of the sections to bear cilia upon their surface. In places where the sections are cut transversely and obliquely, these cells are observed to have an hexagonal outline or section, the mass of them presenting just the appearance of a honeycomb seen from above. No nuclei are to be found in the transparent part of the cells. At their outer or attached extremity, these cells are united with the very deeply tinted layer spoken of above, which can now be made out to consist of rounded and cup-shaped nuclei, one of these belonging apparently to each transparent cell. The large cavity is lined by these nuclei only; the transparent epithelium, which doubtless formed part of its wall originally, having been shed. The external surface presents a layer of papillæ, closely packed together, but distinctly recognisable. They are made up of large cells of a rounded but irregular shape, and do not appear to have any bloodvessels passing into their interior. In some parts of the surface these cells are more distinct, and present the characters of pavement epithelium, having a large and distinct nucleus. The bloodvessels of the tumour are most numerous near the junction of the papillary covering with the rest of the growth, where they are seen to be dilated, and are distended with blood-corpuscles. Towards the centre of the growth are several blood cavities of considerable size, which are surrounded by a thick layer of fibrous tissue.

This tumour appears to be a true mucous polypus—*i.e.*, to be made up of the elements of the mucous membrane of the part from which it springs, with the glandular element largely developed—and is to be classed as an adenoma simplex; the cavities lined by transparent epithelium are the mucous follicles of the cervix, with a greatly exaggerated epithelial lining. In the largest cavities, it is to be noticed, the transparent part of the epithelium is shed, while the outer or nuclear—*i.e.*, the young and growing—part of it remains behind. The general impression given by these follicles is, that they are lined by a vast number of hollow cylinders, very greatly resembling the cells of a honeycomb in appearance, which continue to secrete mucus until a point of great distension is reached, when they are unable to continue their function, and are shed into the mucus, perhaps to be replaced by a fresh crop from the nucleated portion which is left behind.

The structure of this polypus bears a considerable resemblance to the first one which I described at the last meeting, but in that there was a much greater development of the fibrous element in comparison with the glandular; it was altogether of greater density, and I think might be classed as a fibro-adenoma. In that tumour too the papillary layer on the surface was of a much more exaggerated form, and the papillæ contained distinct bloodvessels. It is worthy of notice that the age of the patients, from whom these polypi were removed, was an advanced one; the first two were over 70, and the last one was 64. It is remarked by Montgomery,[1] that "these small polypi of the os uteri occurring in women of advanced age, especially if they are of the vesicular kind, are often the precursors of a malignant form of disease." In one of the cases mentioned at last meeting, the disease was undoubtedly malignant; and it is probable that in the patient from whom the mucous polypus I have just described was removed, the disease will turn out to be of a malignant character.[2]

ARTICLE V.—*The Systematic Education of the Insane.* By W. LAUDER LINDSAY, M.D., F.R.S.E., Physician to the Murray Royal Institution for the Insane, Perth.

DR HACK TUKE of Falmouth has done well in drawing attention[3] to the Educational operations of the Richmond District Asylum, Dublin. I quite agree with him that a *school* should form a department of every county pauper asylum, and, indeed, of every asylum of any size; for Education is quite as important in the case of the rich as of the poor insane, and I have long ago publicly stated the grounds for this opinion.

It is obvious that, in proportion as it can be shown that experiments, such as those originated and so beneficially conducted by Dr Lalor in Dublin, have not only been made, but proved successful, elsewhere, they are likely to be repeated or imitated in the county asylums of England and other countries, until the practice of educating their population becomes general, as I have no doubt some day it will. It has occurred to me, therefore, to supplement Dr Tuke's paper by a brief notice of what has been done during the last twenty years in the Murray Royal Institution, Perth, as regards the systematic education of its inmates. The operations in question do not appear to have been known to Dr Tuke; at all events, he makes no reference to them. They differ somewhat, both in kind and degree, from those so long and so well carried on by Dr Lalor in Dublin.

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[1] Dublin Journal of Medicine, August 1846.

[2] The patient returned to her home a few days after the operation, and died suddenly shortly afterwards with the symptoms of internal hæmorrhage. There was no sectio.

[3] Remarks "On the Richmond Asylum Schools," in the Journal of Mental Science.

I cannot here go into the general question of the Education of the insane. I do not propose at present to show its advantages by setting forth the arguments that might be adduced in its favour. All I aim at now is to specify shortly the various forms of the educational machinery of the Murray Royal Institution during the last twenty years, by giving mainly a series of facts and dates.

But in doing so I am not to be understood, or misunderstood, as setting up, either on my own behalf or that of the Murray Royal Institution, any claim for priority in the genesis or development of the idea of systematically educating the insane. I have always regarded contentions regarding priority as to discoveries or inventions, or their practical applications, as undignified and puerile. Who first introduced a subject is a matter of trivial moment compared with the practical importance to the world of the discovery or invention itself. If a discovery, invention, or idea is of value in itself, it deserves to be made public and to be applied to general use; and though the credit due to him who first practically applies it may be of an inferior kind to that which belongs to the first propounder of the idea, still, to all who aid in its propagation and publicity, a certain share of credit is due as to public benefactors. There are never wanting literary excavators who dig from literary remains facts to show that this or that discovery or invention, idea or opinion, was made or uttered by, or occurred to, one or more ingenious, far-seeing men long prior to the date when the said discovery or invention, idea or opinion, assumed a public or general importance; and the history of the idea of educating the insane is no exception to this illustration of the Curiosities of Literature. Dr Tuke gives, as the date of the beginning of Dr Lalor's experiments, 1857; but he says that thirty years ago certain American alienists had carried out the same idea in a practical way. He also refers to the operations of the Crichton Royal Institution, Dumfries, and of Dr Browne, for so many years its well-known Physician. But the reference certainly does not do justice to the importance of Dr Browne's labours, considering either their date, number, variety, or nature. I have no copy of the first

Annual Medical Report of the Crichton Royal Institution; but in the second (dated 1841), I find the educational treatment of the patients already referred to (p. 21, *et seq.*); and from that date downwards there is an admirable consecutive history, in the various publications of the Institution, of the gradual development of an educational machinery, which in its way, so far as I know, is even yet second to none to be found in American, British, or Continental asylums. In the *New Moon* for April 1845—a magazine that has emanated monthly from the Crichton Institution since December 1844—an article entitled "The Schoolmaster," referring to a certain Berlin asylum and its school, gives a programme of classes and exercises similar to that published by Dr Tuke relating to the Richmond asylum; and much information regarding the educational operations, both of the Crichton Institution itself, and of other asylums in various parts of the world, is to be found in the columns of the still vigorous *New Moon*.

One of the earliest ideas of my own official life—and one of the ideas most speedily carried out practically—was the *systematic education* of my patients, not only and principally, or in the first instance, as a means of occupying and amusing them in bad weather, in the evenings, or in winter, as a substitute for out-door occupations or recreations; but also as a mode of improving their time and leisure—of providing them with practical accomplishments that might be of use in their after (sane) life. Within a few weeks of my official appointment as Physician to the Murray Royal Institution, I had organized an educational scheme, which was inaugurated in the winter of 1854, and in subsequent years was elaborated or expanded. So far as I was concerned, both the idea, and the mode of applying it practically, were original—that is to say, I had seen nothing of the kind in operation elsewhere. That the education of the insane, as attempted or accomplished in the institution in question, was regarded as a startling innovation or novelty, was shown by the fact, that I soon found I had exposed myself to many prophecies of ill success from—to the sneers and sarcasm not only of—certain of my colleagues at the head of other asylums, who some years later, by the force, more probably of public opinion than of imitation from conviction, adopted a similar policy; but even of the London journals, which did us the honour to criticize our efforts. It may here be added, that our successes not only encouraged other asylums to similar attempts and successes; but the result of our Courses of Lectures, for instance, was such as to lead the townspeople of Perth to make fresh attempts, after many failures, to organize similar series in their City Hall.

In the Murray Royal Institution the educational machinery has been applied or adapted to *all classes of the insane*—the educated, affluent, upper ranks, as well as the uneducated, illiterate, or pauper orders, of patients. The *form* of education has varied in the two series of cases; but the principle was equally applicable to, and equally successful in, both. In both, though in different degrees, the education bestowed was that which may fitly be denominated *recreational.* While the lower ranks had their daily industrial training, with regular evening classes for the three R.'s, the upper orders had their classes by day, and of a more select kind, for the fine arts, the sciences, and the languages. Music and dancing were, however, common to all classes of patients, as were also Lectures and Demonstrations, the use of the Library and Museum, etc., and, in short, most of the educational benefits of the institution.

I. HISTORY AND BIBLIOGRAPHY OF THE EDUCATIONAL OPERATIONS OF THE MURRAY ROYAL INSTITUTION.

In the following publications of the institution, not only are there set forth the details of its educational proceedings, the modes of its special applications of the principle of educating the insane; but the general subject itself is fully discussed.

A. *Annual Medical Reports.*

In the table of contents or classified *index* to these reports for the decennium 1854 to 1864, there is a special section on "Education," p. 5. Education is specially treated of in the

Report for 1854–5 (28th), pp. 17 to 20. In the winter of 1854, "Saturday Evening Lectures" were begun.

Report for 1855–6 (29th), pp. 15 to 19; includes a programme of classes and lectures, p. 17.

Report for 1856–7 (30th), pp. 29 to 35; again includes a programme of classes and lectures, pp. 31–2.

Report for 1857–8 (31st), pp. 30 to 33.

Report for 1858–9 (32d), pp. 21 to 26; includes a third programme of classes and lectures, with the addition of readings, pp. 24–5.

Report for 1859–60 (33d), pp. 13 to 19; includes a fourth programme of lectures.

In 1868, annual reports ceased to be issued. In 1864, the whole pauper population of the institution was transferred to the new District Asylum (for Perthshire), built by the District Lunacy Board at Murthly, near Dunkeld; while, since 1857, our educational transactions have been recorded in the columns of *Excelsior.* By these and other circumstances, such as our now limited number of patients, confined exclusively to the non-pauper classes, and the facilities now existing for attending lectures and other forms of recreational instruction in Perth, which are necessarily much more varied and important than any private efforts of our own could be, *modification* in the form, number, variety, and nature of our educational arrangements has been produced. But my faith in their efficacy and my conviction of their importance are unchanged, unless in so far as they may be firmer and stronger than ever.

B. *Excelsior.*

A quarto journal, begun in 1857, for the purpose *inter alia* of recording the educational proceedings of the institution, and which continues to appear at intervals of not more than a year. It contains also the literary lucubrations—sane and insane—of the inmates of the institution.

No. 1 (1857), gives, under the heading "The Murray Mechanics Institute," the programme of a series of winter evening lectures; and contains also programmes of evening classes and of concerts.

No. 2 (1857), contains specimens of translations from English into German, and from French into English,—as illustrations of class exercises.

No. 3 (1857), gives another translation from the German; and a concert programme.

No. 4 (1858), leads off with full "Reports of our Inspectors of Schools,"—the said inspectors being two chaplains of the institution, one of whom was also a parochial schoolmaster, while the other was a city missionary, and ultimately became a popular military chaplain in India.

No. 6 (1859), has an account of the fifth winter course of lectures; besides programmes of evening classes, of readings, recitations, and concerts.

No. 7 (1859), records the results of a botanical-class excursion with Professor Balfour of Edinburgh; and also gives concert programmes.

No. 8 (1860), contains a report of two lectures by patients.

No. 9 (1860), reports another botanical-class excursion with Professor Balfour; as well as a sixth course of lectures and readings.

Nos. 10, 11, 12 (1861), give a "Report on the Progress of the Evening Classes during session 1859-60;" and a concert programme.

No. 13 (1861), has a "Winter programme" of lectures, for the session 1861-62; an account of the Institution "Museum of Local Natural History;" and another concert programme.

Nos. 14, 15 (1862), contain a report on the classes and lectures of session 1861-62; with the account of a dramatic performance, and of a concert.

Nos. 16, 17 (1863), have a detailed account by a patient of a "Museum Conversazione," as well as another notice of a concert.

No. 18 (1863), continues in detail a report of the proceedings at a "Museum Conversazione," in November 1872; and gives a series of Tabular "Statistics of Classes and Demonstrations," for sessions 1860-61-62.

Nos. 19, 20 (1864), again give a concert programme.

Nos. 21, 22 (1865), contain a history of the "Scientific Demonstrations," especially in botany and zoology, given in the institution since 1854; as well as a description of the "Library Catalogue."

Nos. 23, 24, 25 (1866): an account of "Our Contemporaries" includes a résumé of the educational operations of the institution from 1854 to 1866.

Nos. 26, 27 (1866), describe the "New Museum," and the mode of its "Inauguration."

No. 28 (1868), gives an account of certain "Drawing-room Concerts," and the practisings involved.

No. 29 (1869), contains a similar report of "Drawing-room Dramas;" a description of the Museum "Guide;" and another concert notice.

No. 30 (1870), describes another "Conversazione," which included, like its predecessors, an exhibition of scientific apparatus and books, and of natural history specimens; and records certain "Additions to the Library" since the publication of the catalogue in 1863.

No. 32 (1872), leads off with a "Prologue," delivered by a patient, at the opening of a series of winter concerts; gives the programme of a "Soirée musicale" (concert); and records further "Additions to the Library."

No. 35 (1875), again notices "Additions to the Library."

Throughout "Excelsior" are given "Circulation lists" of various newspapers and serials (*e.g.*, in No. 35, 1875); while its "Chronicle" records many of the educational transactions of the institution that have not been specified under the head either of "Annual Medical Reports" or of "Excelsior." It mentions further the lectures, and other sources of instruction, *in town*, of which the inmates are constantly taking advantage, especially in winter.

II. FORMS OF EDUCATION ADOPTED IN THE MURRAY ROYAL INSTITUTION.

1. *Industrial Training.*—For instance: shoemaking; tailoring; carpentry; painting.

2. *Classes.*—Both day and evening, on Sabbaths as well as on week days, including monitorial instruction, the subjects taught being chiefly the following:—reading, writing, arithmetic, geography and history, Bible knowledge; vocal music, sacred and secular; needlework, drawing, dancing; the modern languages, French and German; cookery.

Under this head are to be included—(*a.*) *Class Examinations or Inspections.*—Usually by the chaplains of the institution, who acted as inspectors of schools. (*b.*) *Competition.*—With prize gaining and giving.

3. *Lectures.*—Scientific and literary.

4. *Demonstrations.*—Mostly scientific, microscopical, or other; including *Object-teaching.*

5. *Readings and Recitations.*—Mostly literary.

6. *Field-Excursions and Garden Parties.*—Botanical, zoological, and geological.

7. *Chapel Services*, 3 times a week, by a chaplain, who is one of the clergymen of Perth.

8. *Concerts* and musical rehearsals.

9. *Dramatic* representations.

10. *Magic Lantern* exhibitions—as illustrative of natural philosophy and natural history.

11. *Conversazioni*, scientific and literary.

12. *Library*, General; with a *Catalogue raisonné* of its contents, published in 1863. This catalogue was drawn up by one patient, an Episcopalian clergyman, and thoroughly revised five years subsequently by another patient, a solicitor, both of them men remarkable for their habits of method and accuracy.

13. *Museum* of (*a*) Natural History, (*b*) Archæology, (*c*) Technology, and (*d*) Numismatology. Established in 1855; with a *Guide*, published in 1868, a small 8vo pamphlet of 36 pp., containing (*a*) An Introduction as to the objects and uses of museums, (*b*) A General classified catalogue of the contents of the museum, and (*c*) A Bibliography.

14. *Literary Gazette*, "Excelsior."

15. *Circulation* of newspapers and serials of the best class, such as the "Scotsman," "Daily Telegraph," "Athenæum," "Nature," and "Good Words."

16. *Attendance* at classes and lectures, concerts and dramatic representations, with the use of the public libraries and library clubs, *in town*.

III. KIND OF TEACHERS EMPLOYED IN THE MURRAY ROYAL INSTITUTION.

1. *Educated Patients* of both sexes.

2. *Officers* of both sexes and of all grades; including the physician, assistant physicians, matron, housekeeper, head (male) attendant, and certain of the ordinary attendants. Patients and officers frequently acted conjointly, and also along with various classes of

3. *Strangers*—including—

(1.) (*a*) Professional Teachers, such as certain University Professors, *e.g.*, Professors Blackie and Balfour of Edinburgh; Cleland of Galway; Lawson of Halifax, Nova Scotia.

(*b*.) Other academical Teachers, *e.g.*, Dr Miller of the Perth Academy; Dr Schaefer of the Madras College, St Andrews; Mr David Kennedy, father of the well-known "Kennedy family" of Scottish vocalists.

(2.) Other men eminent in various departments of science or literature, *e.g.*, the late Alexander Smith, the poet; the Rev. Dr Wallace of Glasgow; Sheriff Barclay of Perth; Andrew Murray, F.L.S., of London, equally distinguished as an entomologist, botanist, and general zoologist; Mr Croall of the Smith Institute, Stirling, the author of the beautiful vols. of "Nature Printed British Seaweeds," and of the "Plants of Braemar," drawn up at the instance and for the use of Her Majesty the Queen.

ARTICLE VI.—*Observations demonstrating the Influence of Digestion in the Mother upon the Frequency of the Fœtal Pulse.* By Mr CHARLES G. R. NAYLOR.

(*Communicated by* Dr MATTHEWS DUNCAN *to the Obstetrical Society of Edinburgh,* 12*th January.*)

I MUST state, by way of premise, that these observations have been made with the utmost care and accuracy, every effort having been put forth to prevent the occurrence of any excitement of the circulation in the mother after meals, either by exercise or otherwise. The woman was requested to remain in bed and in the recumbent position, with all her upper garments removed, for the space of at least half-an-hour prior to the performance of each auscultatory examination.

It will be seen that the articles of diet have been varied, since the observations were conducted before and after dinner, tea, and supper. The period, two hours after each meal, has been selected for examination, owing to the fact that digestion is at this time thoroughly established, although not completed.

With these few preliminary remarks I shall now proceed to place before you the various records of five cases upon whom my observations were made, during the prosecution of my duties as Resident Surgeon in the Maternity Hospital of Edinburgh.

CASE I.—Anne Kennedy, æt. 19 years; servant girl; primipara. Menstruated last in January 1875, and was delivered of a living and apparently healthy child on the 21st October. Examined after admission, the fœtal heart-sounds were heard most distinctly at a point 1 inch to the right of the umbilicus, in a line between it and the anterior iliac spine. The patient's temperature and pulse are normal.

The following is the tabulated record of the examinations, which were made half-an-hour before and two hours after each meal:—

	Ante Dinner.	Post Dinner.	Ante Tea.	Post Tea.	Ante Sup.	Post Sup.
Sept. 9	136 per min.	152 per min.	136 per min.	156 per min.	144 per min.	148 per min.
	Dinner — Beef and potatoes.		Tea—Bread, butter, and tea.		Supper — Porridge and milk.	
Sept. 13	128 per min.	148 per min.	136 per min.	152 per min.	132 per min.	140 per min.
	Dinner — Boiled beef and potatoes.		Tea—Same as last day's.		Supper — Porridge and milk.	
Sept. 14	128 per min.	152 per min.	136 per min.	(Engaged.)	132 per min.	144 per min.
	Dinner—Broth, meat, and bread.				Supper — Porridge and milk.	
Sept. 16	140 per min.	148 per min.	(Otherwise engaged.)		128 per min.	144 per min.
	Dinner—Irish-stew and bread.				Supper — Porridge and milk.	

Case II.—Margaret Meek, æt. 23 years; multipara, this being her second pregnancy. She is and has generally been a healthy woman.

Fœtal heart heard pulsating an inch above the umbilicus, and most distinctly on the linea alba.

	Ante Dinner.	Post Dinner.	Ante Tea.	Post Tea.	Ante Sup.	Post Sup.
Sept. 13	136 per min.	152 per min.	140 per min.	160 per min.	Fœtal movements too constant to allow proper estimation of the frequency.	
	Dinner — Boiled beef and potatoes.		Tea—Bread, butter, and tea.			
Sept. 14	136 per min.	148 per min.	132 per min.	160 per min.	Fœtal movements too rapid again to-night.	
	Dinner — Broth, meat, and bread.		Tea—Bread, butter, and tea.			
Sept. 16	136 per min.	152 per min.	(Otherwise engaged.)		132 per min.	148 per min.
	Dinner—Irish-stew and bread.				Supper — Porridge and milk.	

Case III.—Mrs O'Brien, æt. 29 years; multipara (third). Menstruated last in February 1875, and delivered on the 27th October. She carried her last child to the full term of pregnancy, but the first was aborted.

	Ante Dinner.	Post Dinner.	Ante Tea.	Post Tea.	Ante Sup.	Post Sup.
Sept. 22	136 per min.	144 per min.	(Engaged.)		(Engaged.)	
	Dinner—Hashed beef, potatoes, and bread.					
Sept. 23	124 per min.	148 per min.	(Engaged.)		128 per min.	152 per min.
	Dinner—Boiled mutton, potatoes, and bread.				Supper — Porridge and milk.	
Sept. 24	132 per min.	144 per min.	(Engaged.)		124 per min.	136 per min.
	Dinner — Soup, beef, and bread.				Supper — Porridge and milk.	

Case IV.—Christina Forrest, æt. 32 years; multipara (fifth). Menstruated last in January 1875, and was delivered on the 28th of September. She carried all her children to the full term. Fœtal heart heard pulsating 1 inch below the umbilicus on the linea alba.

	Ante Dinner.	Post Dinner.	Ante Tea.	Post Tea.	Ante Sup.	Post Sup.
Sept. 22	140 per min.	152 per min.	(Engaged)		(Engaged.)	
	Dinner—Hash, bread, and potatoes.					
Sept. 23	140 per min.	152 per min.	(Engaged.)		132 per min.	148 per min.
	Dinner—Boiled mutton, potatoes, and bread.				Supper — Porridge and milk.	
	128 per min.	140 per min.	(Engaged.)		136 per min.	140 per min.
	Dinner — Soup, beef, and bread.				Supper — Porridge and milk.	

Case V.—Fanny Brown, æt. 19 years; primipara. Menstruated last in February 1875. The woman is a healthy dairy-maid. Fœtal heart pulsations heard most distinctly 2 inches to left of, and 1 inch below the level of, the umbilicus. Patient's temperature and pulse are normal.

	Ante Dinner.	Post Dinner.	Ante Tea.	Post Tea.	Ante Sup.	Post Sup.
Oct. 10	128 per min.	160 per min.	132 per min.	156 per min.	136 per min.	152 per min.
	Dinner — Hash and bread.		Tea—Bread, butter, and tea.		Supper — Porridge and milk.	
Oct. 11	128 per min.	140 per min.	(Engaged.)		(Engaged.)	
	Dinner—Irish-stew and bread.					
Oct. 12	128 per min.	140 per min.	136 per min.	160 per min.	128 per min.	156 per min.
	Dinner—Broth, boiled beef, and bread.		Tea—Bread, butter, and tea.		Supper — Porridge and milk.	
Oct. 15	124 per min.	144 per min.	128 per min.	160 per min.	128 per min.	136 per min.
	Dinner — Hash, soup, and bread.		Tea—Bread, butter, and tea.		Supper — Porridge and milk.	

ARTICLE VII.—*On the Treatment of a Severe Form of the so-called "Paludal Fever," with Icterus and Renal Hæmorrhage.* By J. LABONTÉ, M.D., Port Louis, Mauritius.

WE must, in those cases, take into account the diathesis of the patient, for, as a rule, fever in that form is only met with in the hæmorrhagic diathesis. All the cases we treated tend to establish this fact in an absolute manner.

As in the milder forms of the fever, there is nothing very definite in its symptoms. Sometimes fever is abruptly ushered in by the "shakes;" at other times the symptoms are insidious. For some days previous to the attack the patient feels uneasy, and experiences a sort of lassitude; his appetite fails, there is anorexia and insomnia, notwithstanding which he goes about his daily occupations, until fever, getting the better of him, forces him to his room by rigors and shakes, more or less intense, and more or less prolonged. As usual in those cases, an abundant perspiration shortly breaks out, to the great relief of the patient. When next micturition begins, the first urine, rather scanty, is of a dark-red colour, passing gradually through a series of changes from dark to pale red, and so on, until it becomes normal, after many hours have elapsed from the time of the attack. Such, however, is not the natural nor regular progress of the case at all times. We most frequently observe, before the first attack has had time to subside, another attack setting in of a more severe and intense character, with urgent nausea and bilious vomiting. The patient is anxious and restless; complains of pain at the pit of the stomach and in the region of the loins; his pulse is quick; tongue furred; skin hot, and commonly dry; sclerotics tinged with bile; and thirst great. Drinks taken with avidity are as immediately thrown up, and when micturition begins the urine is just as bloody as before. Should subsequent attacks follow closely upon each other, then all the above symptoms become more manifestly severe: the bilious vomiting is more urgent; the icteric tint is no longer limited to the sclerotics, but extends to the tegument generally; there is prostration and a semi-comatose state verging upon typhoid, which, when once manifest, is very unfavourable as regards prognosis. On examination, the liver is felt enlarged, pressure over that region causes pain; there is also felt enlargement of the spleen; pain in the lumbar region; bowels are constipated; and not uncommonly a temporary suspension of the functions of the kidneys is observed.

Each stage of the fever has its appropriate treatment.—1*st*, As it is almost impossible, or at least very difficult, to determine a fresh attack, all we can do in the beginning is to prescribe iced, acidulated, or alkaline drinks, as the case may be. If the hæmorrhage continues in the absence of a second attack, counter-irritation by mustard poultices in the region of the loins will suffice to stop the bleeding.

2*d*, Methodical treatment may safely begin after the second attack. In practice, there can be no hesitation to administer to the patient, at least three hours prior to the next outbreak, equal parts of the sulphate of quinine and gallic acid, combined with opium or hyoscyamus; iced drinks are continued in small quantities. For bilious vomiting, a dose of ipecacuanha is of service. Nausea and hiccup are to be treated by the appropriate means, even by a blister to the epigastric region when urgent.

3*d*, If the bleeding continues, and the fever take the continued type, large blisters to the calves do good by reflex action. When they fail in their effect at a distance, there can be no hesitation as to the propriety of their direct application to the lumbar region. This has not, in so far as I am aware, been followed by any bad results.

One, two, or at most three doses of the above compound are generally quite sufficient to abate all the most alarming symptoms and to bring on convalescence, during which purgative antibilious medicines are to be given freely. The continued form of the fever lasts on an average from two to four weeks, during which medical treatment must vary according to the circumstances of the case.

As soon as the hæmorrhage has stopped, iced acidulated drinks and blisters are to be discontinued. Cinchona and ratanhia in combination are then to be preferred to the sulphate of quinine and gallic acid. Neurine tonics are seldom to be dispensed with. As a cholagogue, decided preference is due to calomel, podophyllin, and to the extract of taraxacum, combined with the addition of small quantities of quinine. Temporary suspension of the functions of the kidneys does not warrant the use of the catheter, a practice quite as unnecessary as it is injurious. I never saw uræmia occur in any case of that sort, but, under proper treatment, the flow of urine never failed to return after the interval of some hours.

I do not mean to say that such treatment is infallible in the form of fever under consideration; but this much is certain, that deaths have been quite the exception in the very numerous cases that I have as yet seen, and where this plan of treatment was carefully and completely carried out. Common among men, this form of fever is comparatively rare amongst women; infancy is no protection against it; nor is one attack prophylactic against another—with this remarkable feature, however, that it returns periodically, and is either annual or biannual. Neither do the seasons of the year seem to have any influence on the disease, nor the different districts of the island, nor has it appeared evident that one race of men is more exempt than another.

Another form of the fever, closely allied to the former, is that in which, in the absence of icterus, there is abundant submucous and subcutaneous hæmorrhage at the same time. Such cases might pass for "purpura hæmorrhagica." Patient complains of great pain all over; there is prostration, and a feeling of malaise, with constant drowsiness. There is not a region of the body where the subcutaneous hæmorrhage may not take place; internally, the buccal and intestinal mucous membranes are its most common seat. At the mouth it causes great suffering and annoyance, and in the abdomen it causes griping pain, with profuse bloody discharge. The sulphate of quinine in those cases does not answer well. The perchloride of iron and preparations of bark act with the greatest certainty. Acidulated drinks and gargles are powerful auxiliaries, as well as opium, when there are signs of irritability of the system.

ARTICLE VIII.—*A Case of Pleuritic Effusion Cured by Antiseptic Treatment.* By ANDREW MARSHALL, M.D., Hon. Med. Officer to the Preston and County of Lancaster Royal Infirmary.

JOHN TURNER, æt. 28, a butcher, from Accrington, was admitted into the Infirmary under my care on the 21st July 1873. He stated that he had bad eyes when a child, but with that exception had always enjoyed good health. When at work, three days ago, he fell, and the point of his knife wounded his chest. Considerable bleeding occurred. A doctor was consulted, who probed the wound, and said the cavity of the chest was not penetrated.

On admission, he was thin, pale, and weak. There was a small wound, nearly healed, in the fourth intercostal space, one inch and a half inside of the right nipple.

23d July.—There was pleurisy with moderate effusion on the right side.

31st.—There was dulness all over the right side, extending beyond mid-sternum. The apex of the heart beat between the fifth and sixth ribs, one inch and a half outside the left nipple. The liver was depressed. The right side measured one inch and a quarter more than the left. The patient was very feeble. Had a distressing cough and orthopnœa. Pulse 120; temp. 102·6°. Forty-eight ounces of sero-sanguineous fluid were drawn off by Dieulafoy's pneumatic aspirator. To have tr. ferri perchlor., good nutritious diet, and a moderate quantity of brandy; tr. iodi to be applied to the side. The operation afforded great relief, and the patient's condition gradually improved until 10th August, when he had a rigor, followed by a return of all the symptoms. Pulse 152; resp. 52; temp. 104°.

14th August.—Paracentesis was considered necessary, and sixty-one ounces of fluid, slightly tinged with blood, were evacuated. A mixture containing the acet. and bicarb. potassæ, and iodidi potassii, was ordered.

23d.—The fluid had re-accumulated. Patient was so weak that he could scarcely speak, and could not feed himself. Sixty ounces of serum were drawn off. Decided temporary relief resulted, but no permanent improvement.

30th.—Paracentesis was again resorted to, and eighty-six ounces withdrawn. Ferri et amm. cit. and digitalis prescribed.

5th Sept.—Tapping was again required, and seventy ounces were removed.

11th.—Patient was extremely low; in fact, he seemed to be rapidly approaching his end. The effusion was discovered to be purulent. Accordingly, with a bistoury, I made a free opening between two of the lower ribs, beginning about two inches from the spine, and cutting outwards. One hundred and thirty ounces of pus escaped. A wide drainage-tube was then inserted through the wound, and retained in position by strong carbolized silk passed through its outer end transversely, and tied round the chest. All this was done under a cloud of carbolic-acid spray. The wound was then dressed antiseptically, according to Professor Lister's plan, and carbolized tow placed along the lower margin of the dressings, and on the bed, to receive the discharge. From this date the patient improved uninterruptedly. On the day after the operation, the flow from the wound was very profuse, and the dressings were changed twice. After that they were changed daily for a few times, and then the intervals between the dressings were lengthened as the discharge diminished. The discharge remained sweet through the whole course of treatment.

4th Oct.—The tube was finally withdrawn.

5th.—Patient got up and walked about.

1st Dec.—Dr Blair, senior house-surgeon, reported that "The patient has been perfectly well for a month now, and has simply been retained in hospital as his home is not a good one."

He is still in good health, and regularly follows his employment.

For pleuritic effusions defying other modes of treatment, paracentesis thoracis is a safe and generally a successful operation. "The removal of just as much fluid as may be necessary to relieve substantially the mechanical distress, will, in most cases, give the necessary spur to the natural process of absorption, by means of which the rest of the fluid will be taken up." But cases are occasionally met with where paracentesis fails, and to such the method adopted in Turner's case is very applicable. It is, if properly carried out, at least as safe as paracentesis, and would probably be found more successful. In all cases of empyema, when the fluid is not fetid, it is, I think, the best treatment that can be adopted.

ARTICLE IX.—*Sclerosis in Scattered Patches.* By Prof. CHARCOT, La Salpêtrière, Paris. Translated by THOMAS OLIVER, M.B., Preston, with M. Charcot's permission.

(*Continued from page* 726.)

LET us now see what direct observation permits us to recognise in the thin sections of the cord, hardened by chromic acid and coloured by carmine. As in the case of the stroma of lymphatic glands, which we took an instant ago for an example, it is important to distinguish here, in the first place, cells, and in the second, a network of fibroid trabeculæ, which bind these cells with one another. The question now for consideration is, What is seen in the white substance ?

The points of the reticulum at which several trabeculæ meet, form here and there protuberances or *knots* more or less thick, situated at nearly equal distances from one another. Now, each of these knots, those especially which are prominent from their great size, present towards their centre a figured body, rounded or slightly oval, more deeply coloured by the carmine than are the adjacent parts. These bodies are nuclei, with a clear outline, finely granular, destitute of a nucleolus, and measuring on the average from 4 to 7 millimetres. They are seen to be soluble in acetic acid, which makes them contract in all directions, and diminishes their diameter sometimes by one half; they are known by the name of *myélocytes* (Ch. Robin) or *nuclei of the neuroglia* (Virchow). A thin layer of protoplasm, without distinct cellular appearance, very generally surrounds these nuclei (myélocytes, nucleated variety), which, at other times, on the contrary, are shut up in a truly rounded or star-shaped cell (myélocytes, cellular variety), and provided with prolongations more or less numerous (from three to ten according to Frommann), and more or less stretched out. The prolongations appear to be incorporated with the trabeculæ of the reticulum, which continue them, so to speak, without any appreciable line of demarcation; in the case in which the cellular form is not distinct, the nuclei, naked, or covered only by a thin layer of protoplasm, appear as centres, from which arise the trabeculæ of the reticulum, and from which they radiate in various directions.

The trabeculæ must be studied in their turn, and considered independently of the connections which they may have either with the nuclei or with the cells which occupy the knots of the reticulum: their texture varies somewhat according as we examine transverse or longitudinal sections. In the first case, they simulate homogeneous thin partitions, brilliant, and of a fibroid appearance. In anastomosing, they form meshes of which the narrowest are still broad enough to contain a nerve-tube. But what of the longitudinal sections? We see the trabeculæ ramify nearly to infinity, and produce a network with much smaller meshes. This network is, besides, arranged under the form of partitions, which separate the nerve-tubes from one another, and surround them after the manner of a sheath. The empty spaces which exist here and there between the sheaths and nervous tubes, seem filled up by a little quantity of amorphous matter, finely granular. In no part do we meet, in the normal state, in the midst of these trabeculæ, the thin fibrils which form an integral part of laminous tissue.

In the grey substance, the neuroglia is formed upon the same general plan, only the meshes of the fibroid network, especially at the points where the nervous elements are awanting, are there more densely packed than in the white substance; and thence results the spongy aspect which we have already mentioned. Let us add, that the radiating cells seem more numerous than anywhere else in certain regions of the grey substance, and that they are sometimes so developed that it becomes very difficult to distinguish them from nerve cells; but we shall have occasion to speak at large on this point.

A dense fibroid network, with narrow meshes, and numerous cells are found in the parts of the white bundles in which no nerve-tubes exist, in the cortical layer (Rindenschicht) for example, and in the broad partitions which take their origin from it.

If we refer to the preceding description, the neuroglia undoubtedly deserves to be attached to the type of reticulated connective tissue, whose essential characters we touched upon a short time ago.

But this description has been principally traced (you have not forgotten it) from observations made upon fragments of the cord which have been subjected, during a more or less lengthened period, to the action of chromic acid. Now are the results obtained by this method of preparation beyond the pale of criticism? Such is not the opinion of some authors, amongst whom we must cite, in the first rank, masters such as Henle and Robin. According to them, the fibroid reticulum described above would have no real existence; it would be a production of art. In the fresh state, before the intervention of reagents, the spaces intermediary to the nerve-tubes would be filled, not by solid trabeculæ, forming by their arrangement the meshes of a network, but simply by a soft, greyish, finely granulated, amorphous matter, in the midst of which the myélocytes would be, as it were, suspended.

This matter having the property of becoming hardened, without loss of volume, under the influence of alcohol and various acids, of chromic acid in particular, it is to this circumstance that would be owing the fact of its presenting itself, in the preparation treated by this last agent, under the form of a reticulated apparatus. To these objections have been opposed arguments, or rather facts, some of which, we think, have a value almost decisive of the question. It is admitted that there exists in the normal condition, interposed between the nervous elements (in small proportion, it is true) amorphous matter, possessing the characters which have just been indicated (Kölliker): we know equally that in fresh pieces, the reticulum is less clearly delineated than in pieces hardened by acids. But it is not the less true, that even in the fresh state, thin sections of the white substance of the spinal cord, placed in iodized serum, and lacerated under the microscope, allow you clearly to see upon their edges the fibroid tractuses of connective tissue (Kölliker, Frommann, Schultze). This result, easy to obtain in normal conditions, is still better seen in certain pathological conditions in which the normal arrangements are seen exaggerated without being radically modified.

It is this which happens, among others, as we shall tell you, in subacute interstitial myelitis, and in sclerosis properly so called when the degeneration has not yet gone beyond the first phases of its evolution.

From all this it has been concluded (and we consider the conclusion quite legitimate) that, in this species, chromic acid has no other effect than to bring into greater relief the reticulated texture of the conjunctive sheath of the spinal cord. This arrangement pre-exists, and is not produced altogether by the action of the reagent.

To finish with the remarks which I ought to lay before you relative to the normal histology of the spinal nervous centre, I have only one word to add regarding an anatomical peculiarity which the smallest blo dvessels offer, especially the arterial capillaries, in the depth of this organ. They possess, like the intra-encephalic arterioles, that supernumerary tunic which is commonly designated under the names of lymphatic sheath, or the sheath of Robin. A free space, occupied by a transparent liquid, in which there float some figured elements, separates, as you know, this sheath from the adventitious tunic. You will soon recognise the interest which is attached to this anatomical arrangement when there is question of interpreting certain lesions.

II.—After these preliminary remarks, it becomes easy to us to enter upon the study of the histological degenerations of the spinal cord in *sclerosis in patches.* The description of these degenerations, which we are about to present to you, will be founded chiefly on the results of investigations to which M. Vulpian and I have devoted ourselves for a long time. We shall have, moreover, several times an opportunity of turning to account, under correction, researches made previously or since that time, upon the same subject by Valentiner, Rindfleisch, Zenker, and especially by Frommann, who, apropos of the examination of a small portion of the spinal cord, has written a large book, accompanied by elegant plates, and rich in valuable papers.

We shall describe, in the first place, what we can see (1) upon transverse sections, and (2) upon longitudinal sections, taken from portions of the spinal cord hardened by chromic acid; we will afterwards describe, in accordance with the examination of fresh pieces, some peculiarities not recognisable in the hardened sections. In the two cases the coloration of parts, produced by the aid of an ammoniacal solution of carmine, will be, as in researches relative to the normal state, an auxiliary of great utility, and which it will be good to bring into operation.

(*A.*) When we examine with the naked eye a piece of the spinal cord cut at random, and bearing a patch of sclerosis, it seems that the diseased portions separate themselves from the healthy parts in a sudden manner, without transition, by a line of demarcation sharply drawn. Now, that is an illusion; microscopic study, in fact, enables us to ascertain, even with feeble magnifying powers, that the portions which are healthy in appearance and which border upon the sclerous nucleus, present within a radius of a certain extent, traces of degeneration already very evident. If we look beyond the apparent limit of healthy tissue, the lesions are found to be more decided and appear in more marked progression, in proportion as we approach the central region of the patch, a region in which they acquire their highest degree of development. In proceeding thus from the peripheral portions towards the central parts, we are led to recognise the existence of several concentric zones, corresponding to the principal phases of the degeneration.

(*a.*) In the *peripheric zone,* we observe what follows: the trabeculæ of the reticulum are notably thickened; sometimes they have acquired a diameter double of that which is found in the normal state. At the same time, the nuclei, which occupy the knots of the reticulum, have become more voluminous; sometimes they are multiplied, and we may count two or three of them, rarely more, in each knot (occasionally some of these nuclei present towards their centre a contraction which seems to indicate a commencement of division); the cellular form is shown there more distinctly in consequence of the thickening of the trabeculæ. The nerve-tubes appear more distant from one another; in reality, they are particularly diminished in volume, and this sort of atrophy has been effected at the expense of the cylinder of myeline, for the axis-cylinder has preserved its normal diameter even where it is hypertrophied. The amorphous matter, which encloses on all sides the fibres of the reticulum, appears more abundant than in the healthy state.

(*b.*) The nerve-tubes in the *second zone,* which we might call the *zone of transition,* have become still more slender. Many of them seem to have disappeared ; in reality, they have only cast off their cylinder of myeline, and are no longer represented except by the axis-cylinder, which, in truth, has sometimes acquired dimensions relatively colossal. As for the trabeculæ of the reticulum, they exhibit degenerations not less remarkable. They are more transparent and their outlines are less decided ; in fine, in certain places (and that is really a fundamental fact), they are replaced by bundles of long thin *fibrils,* very analogous to those characteristic of ordinary laminous tissue. These fibrils are arranged in a manner parallel to the great axis of the nervous tubes ; that is why we scarcely notice anything of them on the transverse sections, except their extremities, which, by their junction, figure as a very fine dot. They tend, as we have said, to substitute themselves for the fibres or trabeculæ of the network ; but, besides, they invade the meshes which contain the nervous tubes, in proportion as these diminish, by stripping themselves of their myeline, and, as a consequence, the reticulated or alveolar aspect, which is so distinct, and which presents in the normal state the conjunctive sheath, tends to become more and more effaced.

(*c.*) It is, as you know, in the *central region* of the sclerous patch that we observe the most marked degenerations. Here every trace of the fibroid reticulum has disappeared ; we no longer meet with either trabeculæ or distinct cellular forms ; the nuclei are less numerous and less voluminous than they were in the peripheral zones ; they have shrunk in every direction, they appear as it were shrivelled, and no longer take under the influence of carmine so deep a tinge ; we find them here and there forming little groups in the intervals which the bundles of fibrils leave between them. The latter, besides, have invaded everything ; they now fill up the alveolar spaces, from which the myeline has totally disappeared. Nevertheless, the axis-cylinders, the last vestiges of the nerve-tubes, persist still in greater number, intermixed with the fibrils ; but they have no longer, in general, that volume relatively large which they had sometimes in the first phases of the degeneration ; the greater part are even lessened to such an extent that they resemble (so that you might mistake them) the fibrillar filaments of new formation, from which, however, we shall soon learn to distinguish them.

Such is the last period of the morbid process in the form of sclerosis which we are now considering ; and this indefinite persistence, so to speak, of a certain number of axis-cylinders in the centre of portions which have undergone, in the highest degree, the fibrillar metamorphosis is, observe, a character which appears properly to belong to sclerosis in patches. It is certainly not observed, at least in the same degree, in the other varieties of grey induration, whether there is question of descending spinal sclerosis consequent upon lesions of the brain, or of that which, occupying primitively the posterior cords, is justly considered as the anatomical substratum of progressive locomotor ataxy.

(*B.*) The results of examination of longitudinal sections confirm in their aggregate the data which have just been explained to you. I can therefore spare you longer details, and confine myself to the following remarks which will make you better acquainted, in some respects, with the fibrillar tissue of recent formation. It is upon sections of this kind that we clearly perceive the characters of this tissue, that we can best appreciate the longitudinal direction of the fibrils, their brilliant aspect which causes them to resemble elastic fibres, and their arrangement under the form of fasciculi, slightly undulated, and always parallel. By gently separating these bundles, we see that the fibrils which compose them are extremely thin, that they are opaque and smooth, that they divide and rarely anastomose, whilst they interlace, on the contrary, and frequently involve each other in such a way as to resemble a species of felting, and that they became tinted with difficulty under the influence of carmine. These last characters distinguish them sufficiently from the axis-cylinders, which, besides, are in general more voluminous, translucent, and never ramify. They can also be easily distinguished from the fibres of the network with which they are sometimes found intermingled, inasmuch as these latter are thicker, shorter, and constantly roughened along their edges with branch-like prolongations. Lastly, they differ from the elastic fibres, which are so often found mingled in the ordinary conjunctive tissue, in one particular character. They swell under the influence of acetic acid, and form a transparent hyaline mass, which does not occur with elastic fibres.

Can we enter further into the study of these fibrils and seek to understand their mode of formation? Are they produced, for example, as M. Frommann would have it, partly in the thickness itself of the fibres of the reticulum, which they are destined soon to replace, partly at the expense of the cells and the nuclei of the neuroglia? Do they spring, on the contrary, as others think, either from pre-existing amorphous matter or a newly-formed blastema? Is there, in other words, metamorphosis or substitution? The question, we think, must still remain undecided. All that we can say in this regard is, that the fibrils have appeared to us to spring from the substance of the nuclei or of the cells, and that this fact, if confirmed, might be appealed to in support of the thesis maintained by M. Frommann.

I cannot pass over in silence the various degenerations which the bloodvessels undergo which traverse the patches of sclerosis, degenerations which may be well studied upon the longitudinal sections, after hardening by chromic acid. At the origin, that is to say in the peripheral zones, the coatings of these vessels, even those of the finest capillaries, seem thicker, and contain a greater number of nuclei than in the normal state. Nearer the centre of the patch the nuclei have become still more multipled; and, moreover, the adventitious tunic is found replaced by several layers of fibrils, in all respects similar to those which are developed simultaneously in the thickness of the reticulum. Finally, at the last stage, the walls have become so thick that the calibre of the blood-vessel is found notably diminished.

I ought to point out also in passing, the habitual presence of a certain number of amyloid bodies in the middle of the fibrillar tissue. But I ought to call attention, at the same time, to the singular fact, that these bodies are always less abundant in the sclerous patch than in the other varieties of grey induration.

(*C.*) It is not always without difficulty that we succeed in finding, upon the pieces which have not been prepared by chromic acid, all the details which I have just particularized. On the contrary, fresh pieces offer this advantage, that they allow certain degenerations to be ascertained, which would certainly pass unnoticed if we confined ourselves exclusively to the examination of hardened pieces. I allude here to the existence of globules and granulations of a greasy or medullary appearance, which we meet with almost constantly in more or less considerable number in the thickness of the sclerous patches, in the fresh state, and which soon disappear without leaving traces when the preparation has remained some little time in chromic acid. Now, the presence of these fatty granulations is connected with an important phase of the morbid process—I mean the destruction of the nerve-tubes. Yet, before entering into the developments relative to this point, I consider it useful to go a little further back and recall to your remembrance by a summary description, in which I wish to seek especially terms of comparison, the modifications of structure which the peripheral nerves undergo when, by a complete section, they have been separated from the nerve-centres.

By way of preface, I will remind you, that in the peripheral nerves the nerve-tubes are essentially formed, as in the spinal cord, of a cylinder of medullary matter or myeline, and of an axis-cylinder; but that they possess besides a sheath of connective tissue, the sheath of Schwann, which, according to the most recent researches, appears not to exist on the more slender tubes of the nervous centres, or at least to show itself there only in the rudimentary state. You will discover in a moment that this anatomical peculiarity, insignificant in appearance, is not devoid of interest under the point of view that now occupies us.

Here is now the indication of the phenomena to which I have wished to call your attention. Eight or ten days after section of the nerve, there is produced a kind of coagulation of the medullary substance of the nerve-tube, in little masses, more or less irregularly globular, with wavy margins, dark, presenting a double contour, and having consequently preserved all the optic characters of the myeline. On the following days, the segmentation still progressing, the sheath of Schwann of each nerve-tube soon encloses no longer irregular masses of myeline, but, on the contrary, drops presenting the appearance and micro-chemical characters of grease. These drops, at first rather large, become progressively, in consequence of division, which continues to occur in them, smaller and smaller, and finally they are replaced by very fine granulations resembling dust, which would fill up the conjunctive sheath. Paler granulations of a Proteus-like nature are found in certain proportions mixed with the preceding. Lastly, globules and granulations disappear, and the sheath of Schwann, reflected on itself, is folded so completely, that when we examine a certain number of juxtaposed nerve-fibres thus degenerated, we should think we saw upon the field of the microscope a bundle of filamentous connective tissue. What becomes of the axis-cylinder during this time? Composed specially of Protean matter, it for a long time resists the action of the causes which have destroyed the myeline, for we can recover it again sometimes in the sheath several weeks or even months after section of the nervous trunk.

To recapitulate: in the conditions of recent nutrition in which the nerve-tubes are found placed in consequence of section of the nerve, the medullary matter coagulates, then disintegrates, and gives birth on one side to Protean molecules, on the other to corpuscles, which preserve at first the appearances of myeline, but which, in consequence of an ulterior modification, soon present all the characters of droplets or fatty granulations.

Let us now return to the patches of sclerosis. There we have to study phenomena at least very analogous to those of which I have just spoken.

In the depth of the sclerotic focus, in fresh pieces, we meet almost constantly, as we have already said, and often in considerable number, with globules or granules offering, in a general manner, the appearance of fat bodies. These bodies present themselves under two principal aspects. Some figure as masses relatively large, whose dark sinuous borders simulate either the form of an irregular oval globule or that of a club—sometimes of a kidney. They offer, like the myeline, with which they are closely connected, moreover, by other characters, a double contour. Others are real droplets or fatty granulations, sometimes free, sometimes agglomerated in such a way as to form confused heaps or coherent aggregates, otherwise called *granular bodies,* deprived of nucleus, and enveloping membrane. Protean molecules are found here and there mixed with these various granulations.

All these products exactly resemble, you see, those which result from disaggregation of the myeline in the case of section of a nervous cord.

Let us follow out the analogy. On the longitudinal sections which I show you, we see at certain points fatty granulations, arranged under the form of long tractuses parallel to the direction of the nerve-tubes. Upon the transverse sections they form here and there small heaps, separated into islets, which correspond almost exactly with the seat of the alveoli. In truth, the granulations have generally gone beyond the limits of the latter, and have spread out into the neighbouring tissues. But that need not surprise us when we know that the nerve-tubes of the spinal cord are destitute of that cellular sheath, or sheath of Schwann, which in the divided nerves contains on all sides the products of the disaggregation of the myeline. The meshes of the reticulum, and the interstices of the fibrils, offer, besides, easy ways by which the drops of myeline, as well as the fatty granulations, may infiltrate themselves and spread to a distance.

Lastly, we will observe that the masses of medullary appearance, and the fatty granulations, are never met with in the centre of the sclerotic patch; that is to say, in the regions in which the fibrillar metamorphosis and the work of destruction in the nerve-tubes have terminated. On the contrary, they always occupy the most external parts of the patch, or, in other words, the peripheral or transition zones. Now, at these points, as you know, the morbid process is in full activity. It is there, in fact, that, being compressed on all sides, and choked by the trabeculæ of the reticulum, which are thickened, and, later on, by the bundles of fibrils, which tend to invade the alveoli, the medullary cylinder progressively decays and then disappears, the nerve-tube being no longer represented at last except by the axis-cylinder. The accumulation of medullary or fatty droplets, and the destruction of the cylinders of myeline take place, therefore, simultaneously. We may even add that they proceed *pari passu*, since the former ceases to be produced when the latter is definitively accomplished. Evidently the coexistence of the two phenomena cannot be casual, and, considering all that precedes, it seems to us legitimate to conclude that the medullary and fatty corpuscles in question are no other than the debris, the detritus arising from the breaking-up of the nerve-tubes.

What becomes, consequently, of these fatty granulations? They in all probability disappear by way of reabsorption. You know that we no longer find traces of them in the central parts of the sclerotic foci. This is the place to point out specially to your attention a phenomenon, which, without doubt, is connected with the phenomenon of this reabsorption. As you may ascertain by the preparations which I am going to exhibit, in the places where the products of this breaking-up of the nerve-tubes are met with, the lymphatic sheaths of the little vessels enclose within their cavity, in variable proportion, either fatty granulations, or even, although more rarely, corpuscles presenting the characters of myeline. At certain points these various products are so numerous that the lymphatic sheaths are distended to excess. The vessels then appear to have acquired a volume double or triple of that found in the normal state, and they appear under the form of little white tractuses, visible to the naked eye, upon the grey background of the sclerous patch. Yet the tunics themselves of these vessels do not offer any other degenerations than those which have been indicated above, and which certainly have no connexion with atheromatous degeneration. In fine, the question here is one of consecutive fatty infiltration of the lymphatic sheaths, and by no means of a primitive lesion of the vascular walls. The same phenomenon is found in softening of the brain from arterial obliteration, in the greater number of forms of sclerosis, primitive or secondary, and, in a word, in very different affections of the nerve-centres, but which, nevertheless, have this in common, that they determine the fatty degeneration of the nerve-tubes. The true character of this phenomenon would seem to have been suspected by Gull and by Billroth, but it has been brought into prominence especially by M. Bouchard, in his beautiful studies on the secondary degenerations of the spinal cord.

The description just presented to you of the degeneration found in sclerosis in scattered patches, has special reference to the white substance, but it can be applied equally in a general manner at least to the grey substance. In both substances, the neuroglia is actually constructed upon the same model, and the degenerations which are produced in it do not differ essentially. In accordance with observations which I have been able to make, I shall mention only the modifications which the nerve-cells undergo, when, in consequence of the invasion of the grey substance, they are found comprised within the area of a patch of sclerosis. These cells are not the seat of a nuclear proliferation, contrary to that which takes place under the same circumstances in the case of conjunctive cells, whose nuclei multiply habitually, and that is even a character which, in case of need, would lead us to distinguish from one another the two orders of elements. They undergo a particular change, which we may designate under the name of *yellow degeneration,* in consequence of the well-marked ochre-like colour which they present. They cease to be brightly coloured by the carmine, as in the normal state. The nucleus and the nucleolus appear formed of a substance of a glassy brilliant aspect. That is the case with the body of the cell, which, moreover, seems to be composed of concentric layers. In short, an atrophy capable of bringing about a relatively great diminution of volume seizes upon all parts of the cell at the same time as the cellular prolongations wither up and disappear.

In the brain, and also upon the optic and olfactory nerves, the sclerotic patches present essentially the same characters as in the spinal cord, and we do not consider it necessary to enter in this respect into new details.

Having reached the term of this study, we may endeavour to re-establish, in the natural order of their succession, the phenomena which compose the degeneration in question, and thus seek to recognise the pathological mode according to which this degeneration sets in.

Unquestionably, the multiplication of nuclei and the concomitant hyperplasia of the reticulated fibres of the neuroglia are the fundamental fact, the necessary antecedent. The degenerative atrophy of the nervous elements is secondary, consecutive. It has already begun to be produced when the neuroglia gives place to the fibrillar tissue, although it proceeds then at a more rapid pace. The hyperplasia of the vascular walls plays here only a subordinate part.

Wherein does the affection of the neuroglia consist which marks the outset of this series of disorders? It is easy to discover in it all the characters of formative irritation. But after having recognised that sclerosis in patches is a myelitis or a chronic interstitial primitive and unilocular encephalitis, it will remain for us to determine the histological characters which distinguish it from other forms of sclerosis of the nervous centres, and thus from several kinds of myelitis or encephalitis, which, taking equally their point of departure from the neuroglia, yet do not end in fibrillar metamorphosis. We will undertake, at a suitable time, to perform this task. For the present, we are in haste to leave the pathological anatomy for the clinical study, and to show you by what set of symptoms sclerosis in patches of the nerve-centres reveals itself.

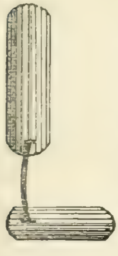
ARTICLE X.—*Reports of Cases treated in Leith Hospital.* By W. A. FINLAY, M.D., M.R.C.P.E., Trinity.

LEITH Hospital has for many years rendered valuable aid to the sick and injured poor, and its usefulness has within the last two years been much increased, a legacy having been left to it by the late William Ramsay, Esq., of Lixmount. This has enabled the managers to build an addition, consisting of four large, well-ventilated, and comfortable wards, besides an operation-room and others necessary for the comfort of the attendants. The Nightingale system of nursing is found to work admirably under the superintendence of the lady who is at the head of it; the staff of nurses having also been increased. In all respects, indeed, the hospital is well worthy of a visit from those who are interested in the treatment of the sick poor.

I have selected the following cases from those which have been under my care during the months of December 1875, and January and February of this year.

Case I.—Henry De la Rue, a young French sailor, admitted 3d December. He had fallen on to the deck from the masthead of a ship in Leith Roads, and had sustained the following numerous injuries:—Fracture of base of skull, of lower jaw, and of right femur, compound comminuted fracture of left femur, also fracture into the left knee-joint, separating the condyles of the femur from each other. Although I regarded his condition as almost hopeless, yet, after setting and applying the long splint to the right thigh, it seemed to me to be my duty to give him the only hope, by amputating the left thigh, which had been so thoroughly shattered; accordingly I amputated about the middle of the femur. He did not, however, survive his fall more than twenty-four hours, although he seemed to improve for some hours after the operation. The fracture into the knee-joint is remarkable, as being exactly between the condyles, and not through one or other of them, as is usually the case.

Case II.—Henry Andersen, æt. 33, a Norwegian sailor, was admitted on 4th December. When at sea, on 19th November, he sustained a compound fracture of both bones of left leg, the limb having been jammed between the ship's side and a boat during a gale. Previous to the patient's admission no splint or bandage of any kind had been applied to the leg, and he described his suffering during fifteen days of rough weather at sea as having been intense. On examination, there was seen on the inner side of the leg a foul sore of triangular shape, each side of the triangle measuring from 3 to 4 inches. This wound communicated with the fracture. I agreed with the doctor who had recommended the case to the hospital, in regarding the injury as being so severe as to render a serious consideration of the question of amputation necessary. Having resolved to save the leg if possible, I removed all the dead bone that could be felt, viz., a splinter about 1½ inches in length, and then laid the leg on its outer side on a concave wooden splint with foot-piece. For the inside of the leg I had a splint specially made, with a steel arch to permit the application of dressing to the wound. The diagram shows the shape of the splint, the wooden parts of which are pieces of common Gooch splints.



These splints having been properly bandaged, a charcoal poultice was applied to the sore. The patient steadily improved from the first, and at the end of ten days, his temperature, pulse, and respirations had fallen to their respective normal standards. The poultices having been discontinued, water dressings and red and other lotions were employed in rotation, a many-tailed bandage being used to compress the sore. On 14th January the wound had quite healed, and the union of the bones was firm. On the 25th of January the splints were removed. He soon began to walk in the ward, and on the 19th February was discharged cured, having, before leaving, on more than one occasion, walked two miles out of doors, and being able to walk fairly without the aid of a staff. On the ninth day after the patient's admission, Professor Spence saw the case with me, and was of opinion that the leg could be saved. The principles which guide a surgeon, in regard to the question of amputation after injury, are such as students at college have very few opportunities of becoming practically acquainted with; and the information gained from lectures, books, and pictures, although sound and proper, does not make up for the want of clinical observation, which in the majority of cases they are unable to make, as the hour which they spend at the hospital is very seldom the one at which such cases are admitted. At the hospital visit of the day following the admission of such a case, they can only see the limb covered with splints and bandages, if it has not been amputated; and if it has, they see it when already off (if they see it at all); and it presents in both these cases a very different appearance from precisely similar accidents which they may be called on to treat after they are legally qualified to practise. There does not appear to be any remedy for this state of things until the probably far-distant day when our students shall reside at our universities; they then could be summoned at any hour when instructive cases of accident were admitted into the adjoining hospital,—unless a certain number of lodgings were built near the hospital to accommodate students, so that all medical students at a proper period of their career might have an opportunity of seeing cases of accident on their admission.

CASE III. *Acute Periosteal Abscess.*—Raymond Stanger, æt. 17, admitted 8th January. Patient is strong and well developed. He is a cabin-boy. The captain of his ship states that the patient has complained of pain when he moved his right arm during the past four or five days. On examination there is seen redness over the right clavicle, extending upwards and backwards round the right side of the neck. There is also some swelling and great pain when pressure is made along the course of the clavicle. Temp. 102·6°.

11*th Jan.*—A swelling, of the shape and size of the top of an ordinary orange, has formed in the angle of the clavicle with sternum. There is fluctuation in the swelling; no bruit or pulsation. I therefore opened it antiseptically, and a considerable quantity of pus was evacuated. Bare bone was felt with probe.

12th Jan.—Patient feels much easier; temp. 99·2°. Next day the dressings were changed under carbolic spray, and there was very little discharge.

17th Jan.—Temp. 97·4°. The thickening over the clavicle is more evident than before. The discharge is slight, and no bare bone can be felt with probe.

20th Jan.—The amount of discharge is considerably greater.

6th Feb.—Patient dismissed at his own request, as his ship was about to sail, there being still a slight discharge from the wound, and the swelling of the clavicle having almost disappeared.

It was unfortunate that we could not retain this patient in hospital until the cure was complete; but, so far as it goes, his case shows the beneficial effects of antiseptic treatment of acute periosteal abscesses.

CASE IV. *Gangrene after Typhus.*—John Hampton, æt. 47, admitted 24th November to the surgical wards from fever-house, having, during convalescence from typhus fever, become the subject of gangrene of the left foot. A line of demarcation having become distinct, passing in front of the ankle-joint over the malleoli at the sides, and near the posterior part of the heel beneath, the gangrene began to affect the constitution of the patient. His pulse and temperature rose, and he was fast becoming weaker. If left to itself, the gangrene would have opened into the ankle-joint, had the patient survived long enough. On the 18th December, I amputated the foot immediately above the ankle-joint, performing the operation by a long anterior flap. This amputation saved the patient's life. He is now (February 1876) sitting up in the ward, his stump not being quite whole as yet, on account of some sloughing of the flap and necrosis.

Amputation immediately above the ankle is very rarely performed, as cases requiring amputation of the foot can usually be treated by Syme's amputation, which, in this instance, was impossible on account of the extent of the gangrene. I should have preferred to amputate immediately below the knee, had it not been that the very weak state of the patient rendered it highly important to remove no more of the limb than was absolutely necessary.

Although cases in which this operation is admissible are rare, yet the fact that it is much less dangerous to life than amputation below the knee, renders it a valuable one. The subject is ably treated in Mr Joseph Bell's "Operations of Surgery." [1]

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[1] Manual of Surgical Operations, by Joseph Bell, page 74.

ARTICLE XI.—*On the Compulsory Intimation of Infectious Diseases.*

By H. D. LITTLEJOHN, M.D., F.R.C.S.Ed., Surgeon of Police and Medical Officer of Health for Edinburgh; Lecturer on Medical Jurisprudence and Hygiene, School of Medicine, Edinburgh.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 1st *March*.)

THE Medical Officer of Health has no greater difficulty than in dealing promptly with the so-called preventible diseases. The only indications which he receives of their presence among us is either the death certificate furnished to him in the returns of the Registrars, which, though handed to him weekly, generally contain not the deaths of the bypast week, but those which have taken place in the preceding one. Thus a fortnight may elapse before important information reaches the sanitary officer of the habitats of disease; and it too often happens that when the locality is visited, the special disease has found other victims, and such an amount of morbific material has been produced that the place has become a prolific centre of infection.

I have no doubt that this has been the experience of medical officers of health in all our cities and towns, and no disease of an infectious character has ever visited Edinburgh that I have not advised the authorities to issue circulars to all the medical practitioners, requesting their assistance, and offering on our part all help in removing the sick and disinfecting houses and clothing. I regret to say that these appeals have received little attention, and that we have been too seldom called upon to aid in preventing the spread of such diseases.

During 1875, the total mortality from zymotic diseases amounted to 873, and this number was distributed as follows:—

Fever—Typhus,	23	
„ Typhoid,	30	
	—	53
Diphtheria,		61
Scarlatina,		509
Measles,		26
Hoopingcough,		204
Erysipelas,		20
		873

Now, allowing the usual percentage of deaths in each form of disease, we can easily figure to ourselves the aggregate of the cases of illness, in all of which I hold that the medical man in charge should have had the assistance of the public sanitary staff; and yet I find that during the year only twelve medical men sent intimations of infectious cases of illness in their practice, and those referred to only eighteen cases; and in a few instances the friends themselves, probably at the instance of the medical men, called, soliciting assistance. Is it to be wondered at that such diseases quickly assume an epidemic form, and entail a large mortality? It may be urged that sanitary measures can be carried out in private without the assistance of any special staff. To this I reply, that practice makes perfection, and gives experience in such operations; and I am confident that the process of disinfection can be more satisfactorily performed by a trained official than by any domestic who has received her directions from the physician in charge. But as no articles of dress or bedding requiring complete disinfection can be received by any establishment in Edinburgh without the cognisance of the authorities, I am enabled to supply the number of houses from which articles were removed by the sanitary staff during 1875. I find that during that year, 134 families or houses supplied articles for complete disinfection. [This number only includes the instances occurring in private practice, and, of course, refers to persons of the better classes.] It is possible that application may have been made for the cleansing of articles from infected houses through other channels than the sanitary office; but then the illness and the nature of the case must have been concealed, because no establishment will receive any article from an infected house without these articles being removed by the authorities, in a conveyance specially set apart for that purpose. The reasons for this precaution are abundantly manifest. First, the risk is avoided of sending home the disinfected articles in an infected conveyance, and, secondly, important information is afforded as to the presence of dangerous diseases. With such numbers before us, it is only too patent that disease has been permitted to multiply comparatively unchecked, and that simple measures calculated to restrict its area have been neglected, to the great risk of the inhabitants.

In the case of a poor family, where several children had bad attacks of scarlatina, occupying successively the same bed, it was ascertained that the bed, after being kept concealed for several weeks, was taken to and emptied in another and better locality of the city. The contents proved to be chaff, and were deposited on a dust-heap. Two children belonging to the neighbourhood played with the chaff, and helped to scatter it. In a few days they both sickened and died of the same disease—scarlet fever. I had no doubt, from the absence of the disease in that locality at the time, that the connexion between the supposed cause and the fatal issue was clearly made out. Again, a lady died in a furnished house of scarlet fever, and as the family belonged to the country, the house was promptly vacated, and the keys restored to the house-agent. My attention was drawn by an anonymous communication to the case; and on visiting and inspecting the premises, I found that the bedding on which the patient had died, and which was discoloured by the discharges, had been locked past in a press, uncleaned and undisinfected. The house itself was in a similar condition, and was ready to be handed over to the first family coming to town for the season.

Such instances, and many more might be given, show the importance of intimation being sent to the authorities, not merely of a death having occurred, but also of all cases of illness from infectious diseases. In that first mentioned, had suitable information been given, the bed would have been emptied of its contents, the chaff destroyed, and, in all probability, its covering too, if it was not deemed worth the expense of thorough cleansing and disinfection; and in such cases occurring among the poor, the authorities willingly restore or renew all articles that have been destroyed. In the second instance, but for an accidental interference of some one, the house with its load of infected material might have been re-let, and much distress and expense incurred in consequence of the carelessness of the parties in charge. And, no doubt, could furniture speak, many others of our furnished lodgings could tell similar tales.

But from the secrecy that attends the presence of infectious diseases in households, and from the want of a settled understanding as to the duty of relatives and medical men in the circumstances, not only does great inconvenience result, but the actual spread of disease is assisted and great annoyance is caused, not to speak of the mortality that too often ensues.

Not very long ago a case of smallpox occurred in a locality inhabited by respectable families of the middle class. The medical man in attendance gave no information to the authorities, no careful isolation of the case was practised, and the bed and body-clothes of the patient were washed in a common wash-house, and exposed on a green plot in front of the houses. In a short time four or five cases of the disease appeared in the adjoining house, and one young married woman died. Now, had the first case been either properly isolated or removed to the hospital, and had the neighbours been properly warned not to communicate with the infected house by inquiring after the sick, etc., and had the clothes been removed by the authorities, disinfected, and then washed, it is highly possible that there would have been but one case of smallpox, and that the death that did occur would not have taken place. It so happened that each of the cases had separate medical men, who were all attending the same locality for a few days before they were aware of the localized outbreak. The duty of the medical practitioner called in to the first case was, in my opinion, plain—viz., to have sent information of the case to the authorities, and thrown upon them the responsibility of taking the steps I have just indicated as best fitted to prevent the spread of the disease. During the present epidemic of scarlatina we have had only too many instances of the risk undergone by the general community from contact with those living in close communication with the sick. Schools have been broken up in consequence of pupils being permitted to leave infected homes and attend school; and also from servants sickening from the disease in consequence of visiting, on their "nights out," infected families in the poorer districts of the town. We have had several outbreaks of the disease in dairies, where, from the want of decision on the part of the medical attendant, and from the want of timeous notice to the authorities, the disease has spread. Large scholastic establishments have in consequence been broken up, entailing a heavy expense to the proprietors, and disturbing the teaching arrangements of a large number of pupils. Here, again, the assistance of the authorities should have been sought, and the dairies should have been shut, so far as the keeping and sale of milk was concerned.

The question comes to be, What is the best remedy for this uncertainty? Who is the party that should be charged with the duty of informing the authorities of the existence of these diseases? It is a question of great importance, and I am anxious to bring it before this Society as one well worthy of its notice. I think the state of public opinion on the subject is such, that even compulsory legislation would be, if not welcomed, at least received with satisfaction. If the condition of our cattle when affected with contagious disease is so narrowly watched, and penalties attached to delay in giving timeous notice of illness, surely it is not too much to expect that ready information of the occurrence of such diseases among ourselves should be demanded, and that under as stringent penalties.

In the case of cattle, it is the owner who is held responsible, and it has been plausibly urged that the father or nearest relative of the human patient should be the party to give information, and who should be liable to punishment and fined for any neglect of this duty. But we must remember that it is part of the training of the farmer and grazier to become acquainted with the diseases of his stock, and he requires no veterinary surgeon to warn him of their presence. He has lived among cattle all his life—his very talk is of bullocks—and the law justly holds him responsible for the neglect of notifying to the local inspector the presence of serious epidemic disease on his farm or in his cattle-sheds. At the present market value of cows, etc., the person who is able to possess a few must be regarded as respectable—he has some amount of intelligence, and he cannot for a moment be compared with the generality of our poor population, especially in the large towns, a large proportion of whom cannot write. And should the head of the family be affected, or several of the children be struck down, it may prove a matter of impossibility for the mother to leave her house, or the father his work, to give the necessary information. Again, it may, in a large proportion of cases, be the interest of the family to conceal the fact of the presence of the illness. A dairyman knows that his business will be for the time ruined should it be suspected that the parties dealing out his milk have been in contact with the sick. A tailor or a dressmaker in similar circumstances knows that he or she would not be allowed to return to their employment, coming as they do from an infected house ; and undoubtedly, in the cases where these tradespeople unfortunately work at home, and where the very material they are forming into clothes becomes a ready means of the dissemination of a virulent poison, every one would shrink from allowing such clothes to be exposed for sale, much less to be sent to their houses to be used by themselves and children.

This tendency to concealment, in circumstances where one's worldly interest is concerned, is so strong, that even the Contagious Diseases (Animals) Act, with all its heavy penalties, proved to be impracticable until a system was introduced of offering a bonus to all who voluntarily gave information, and allowed the infected stock to be summarily dealt with, and thus prevent the spread of infection. And it may surprise some of the ratepayers here present, to hear that, last year, as much as £1200 was paid in this way in the city of Edinburgh.

Now, in the case of infectious diseases in the human subject, the motives to induce concealment are equally strong, if not stronger. What bonus, too, can we offer to the poor tramp or labourer to go a distance to tell what her child is suffering from ? The weather may be inclement, the distance may be great, and we ask an ignorant woman, it may be, who would be better employed nursing her sick child, to leave that child, and, herself a vehicle for carrying infection, to supply information which, I humbly submit, can be more easily and satisfactorily done by another party, who, in many instances, must be the only one capable of giving intimation of some serious outbreak. I mean the medical attendant. In a large number of cases, he is able to make an easy and decided diagnosis which no unprofessional person can, and it often happens that it is in the very worst cases of these diseases that his skill in detecting their true nature is most manifest, and where the friends would be most at fault. I allow that the better classes are more fortunately placed, and by their intelligence, and, with the time they have at their disposal, are better qualified to supply the information which they have received from their medical attendant as to the precise nature of the illness in their families. But, in such circumstances, delays are dangerous. In dealing with infectious disease in a crowded community, hours are of importance; and after a medical man has informed the friends of the existence of the special disease, are we to trust to the slow process of penalties to enforce attention to the requirements of a statute on the subject, when, by an unfortunate delay of a few days, disease is allowed to spread, and important lives may be in consequence sacrificed ?

Among the better classes, too, the greatest risk of delay is precisely in those cases where early intimation would have proved of essential importance to the authorities. Where such cases prove rapidly fatal, owing, it may be, to some special malignancy, the medical man at once retires to leave the family to their mourning; and we suddenly throw upon the parents, in such melancholy circumstances, the duty of intimating to the authorities the fact of the illness. I confess that, from my own point of view, I think the onus of giving this information should rest on the medical attendant, be he fully qualified or simply a medical student; and I have done my best to impress upon our Local Authority the necessity of some such provision in any amendment of our sanitary laws.

The Convener of the Public Health Committee and myself had lately a conference with the Parochial Medical Officers of the City, gentlemen necessarily brought much into contact with the sick poor, and they declared their willingness to report all such cases, provided that their brethren attending the better classes did the same. They averred that it would make a most invidious distinction were the medical attendants on the poorer classes alone to be called upon to discharge this duty, and they protested against any fresh burden being thrown upon the profession, without an acknowledgment that the service asked to be rendered was a medical service, and therefore demanding remuneration. The force of these views was felt by the Corporation, who at once offered to pay 2s. 6d. for each intimation of a case of illness, and with this sum the parochial medical officers declared themselves satisfied.

It has been asked what are the precise diseases that it is proposed to intimate to the authorities. Now there can be little doubt that the list should only include the following:—Fevers properly so called (typhus, typhoid, relapsing), diphtheria, smallpox, scarlatina, measles, hoopingcough, and erysipelas.

As to the mode in which intimation is proposed to be made, I would suggest that every practitioner in the city be furnished with printed slips and stamped envelopes, directed to the Local Authority. The slips bearing the fact of the existence of a specified case of any one of the diseases just named, with the locality carefully noted; and the words printed in legible characters—"No immediate attention required."

To Medical Officer of Health.

Edinburgh, 7th January 1876.

Take notice that there is a Case of

TYPHOID FEVER

at No. 88 *Moray Place.*

NO IMMEDIATE ATTENTION REQUIRED.

On the receipt of such an intimation, the facts would be carefully noted in a register; and if at expiry of say a fortnight no application was made to the office to assist in fumigating, or in the removal of infected articles of clothing, etc., a private note would be sent to the medical attendant reminding him of our willingness to assist in any way he should judge necessary. No visit would be paid to the house without his sanction, and we would simply wait his instructions to act when he should deem it necessary.

If, on the other hand, on looking at the words—"No immediate attention required," we found the "No" obliterated by a stroke of the pen, we should understand that the phrase "*Immediate* attention required," gave intimation to the authorities either that the case should at once be removed to an hospital at their instance and risk, or that there were grave sanitary defects in or about the dwelling which rendered instant investigation necessary, and we should act accordingly.

To Medical Officer of Health.

Edinburgh, 7th January 1876.

Take notice that there is a Case of

SCARLATINA

at No. 111 *High School Yards—2d Flat.*

~~NO~~ IMMEDIATE ATTENTION REQUIRED.

The advantages of this plan are obvious: A practitioner is called in to see a domestic in a private family, ill, say, of smallpox; he might find it difficult to persuade the patient of the advantage to herself and master of her removal to an hospital. Foiled in his endeavour to protect the family from possible danger, he sends the usual intimation of the case, with the addendum "Immediate action required." On receipt of this, armed with the powers we possess under the Public Health Sanitary Act, we would not hesitate to use some degree of compulsion, and remove the case. I am sure that by such a procedure the practitioner's time and temper would be economized. Again, a similar line of remark applies to those cases among the poor where positive refusal is made to the ordinary medical attendant, but where the interference of the sanitary authorities, aided by the police, always proves successful in inducing compliance with the suggestions of the medical man.

In those cases of infectious disease where no medical man has been called in, I would certainly punish the offending parent or relative in charge, unless it could clearly be made out from the circumstances that no special blame attached to them.

Much has been said as to the obloquy attaching to any statute enforced by penalties; now, a statute would be unworkable without penalties, and would leave the duty a matter of option, and the result would be, while one medical man gave every assistance, a large number would shirk their duty, and the enactment would become a dead letter. In a matter of such gravity as the spread of infectious disease, I hold it is the duty of every citizen to give all assistance in his power in checking it. Medical men, from their opportunities, can give special aid, and that too most certainly when most required, viz., at the outset of an epidemic. I have attempted to show the importance to the community of this service being rendered by them, and at the same time that the community is willing to recognise, in some way, these services. They are not onerous, and, when once made compulsory by law, would not be looked on in any invidious light, but would be regarded by the public as a duty which must be discharged by the profession.

They who live in town, and practise among the rich, must remember that this provision, to be effectual, must be made general, and must apply to all, rich and poor, to town and rural populations. Indeed, the arguments that apply to the ease with which intimation of the existence of disease can be made in towns and populous places are valueless when applied to the case of country districts, where the central sanitary authority may be at such a distance that by the poor it can only be reached through the post. How can we expect, in such circumstances, from the illiterate and hard-worked labourer, speedy information of the occurrence of infectious disease? The medical man, on the other hand, has, at the end of his day's visits, merely to fill up such simple schedules as those just given.

The subject is one of great importance, and is daily assuming greater dimensions, so many and so unexpected are the avenues by which these dire diseases reach us; and there can be no doubt that, as they are allowed to spread unchecked, they entail a larger mortality and heavier pecuniary losses. The time has, in my opinion, come when medicine should more actively practise prevention, just as surgery has of late years developed its conservative aspect, and when the practitioner, called to treat a case of infectious disease, must ask himself not merely the question, How best can I effect a cure? but the equally important one, What means should be adopted to protect the family and the community at large from the spread of infection?

Part Second.

REVIEWS.

Seventeenth Annual Report of the General Board of Commissioners in Lunacy for Scotland. Edinburgh: 1875.

THIS is as usual a very elaborate report, and contains a great deal of information which one working at the statistics of lunacy would like to have; but, in common with most documents printed at the public expense, it might be improved by condensation. Many of the statistical tables are well framed and useful, but nobody would complain if others of them were left out.

The total number of lunatics known to the Board of Lunacy is 8069, of whom 6563 are supported from parochial rates. The number of the unreported insane is estimated at 2000, of whom the larger proportion belong to a class little removed from pauperism. The gradual passing of this class into asylums is probably one great cause of the apparent increase of insanity.

One noticeable change in the treatment of insanity is the abolition of airing courts. This measure was first used by Dr Sibbald at the Argyll and Bute Asylum, who, finding his patients could get on well enough without an airing court, never built one; and when he became a deputy-commissioner, he no doubt induced his colleagues to assist in gaining farther trials for the experiment. Airing courts have since been given up in the Inverness, Perth, Fife, and Haddington Asylums, and we understand the superintendents look favourably upon the new system. Generally, the advocates of a change overstate the advantages to be derived from adopting it. We have a strong feeling that airing courts cannot be dispensed with in large asylums situated in populous districts. It might happen too, that while some patients would prefer being walked about guarded and watched by a sufficient number of keepers, this would be irksome and exasperating to others; and that some who might be allowed to go into the airing court would, if taken about the grounds, be so noisy and troublesome, or so determined in their attempts to escape, that they would soon be left under lock and key, and get no open-air exercise whatever. The experiment has only been tried in asylums situated in the country. It would perhaps be well to wait a few years longer before claiming it as finally successful.

The feature which excites interest in the Scottish Lunacy Reports is the system of boarding out insane patients in private houses. This is done principally in poor and remote places, such as the Highlands and islands; for, where the people are well-to-do, they will not take such inmates at low rates. These pauper lunatics are visited once a year. This appears a very small amount of inspection, but it is likely enough that those who take in the boarders would throw up the business if they were exposed to much additional surveillance.

Great credit is due to the superintendents of asylums, who generally select the patients thought fit to be boarded out. It ought to be carefully borne in mind that it is the district and pauper asylums which take in the worst and most dangerous cases—those of acute mania, general paralysis, or determined suicidal insanity—which try so heavily the skill, patience, and attention of the doctor of the asylum, and add so largely to the bill of mortality and record of accidents. The cases in the lunatic wards of poorhouses are easier managed and less likely to sink under the attacks of disease; and those in private dwellings, being the most harmless, not subject to dangerous diseases, and being returned to the asylums when they become unmanageable, or are visited by dangerous diseases, the argument in favour of the system of boarding out pauper lunatics, from their death rate being lower than that of patients in public asylums or in the lunatic wards of poorhouses, seems to us of no value. The mortality of these boarders is given at 5·8 per cent.; in public asylums it is 8·8 per cent. for the males and 8 for the females. In the lunatic wards of poorhouses it is 8·1 for the males, and 7 4 for the females. We think that the Deputy-Commissioners would make their case look better if they showed less anxiety to plead it. They admit that there are evils connected with the boarding-out system, and we do not think these evils so great that they could not afford to state them in set terms.

Dr Sibbald has an interesting paper on the Distribution of Lunacy in Rural and Urban Districts. He thinks that insanity is more common in towns than in country districts, and that this greater frequency occurs in connexion with physical degeneracy of race and excessive mortality.

The Commissioners consider that the grant from Government for pauper lunatics, to diminish the local rates, is not a wise measure, and point out in forcible terms the evil consequences that may be expected to flow from it.

After enumerating the accidents reported to them as occurring in asylums, they observe—" Possibly, the number of accidents among the insane, instead of being diminished, may be increased by congregating them in asylums, a system which is not natural, and which is chiefly recommended by convenience and economy." We do not know what the natural system of disposing of the insane may be, but we are sure that at present asylums are the best system that is practicable. No management will ever be so perfect as to save those surrounded by violent maniacs from occasional injury, or prevent determined suicides from now and then succeeding in hurting or destroying themselves; but we believe that, in the main, the asylums of Scotland, both public and private, are ably managed by hardworking and skilful superintendents, well trained in their profession. It is to the incessant application of their knowledge and judgment that the insane are so well cared for, not by the application of fixed rules. Every one is ready to admit, in theory at least, that when an undertaking is going on well, and is in good hands, liberty ought to be carefully respected. Freedom of treatment is a prime consideration, if a thing is to be done well, and we believe that a superintendent has many means of arriving at a just conclusion, on which an inspector in his hurried visit could neither take up nor be taught. It seems to us, therefore, that the Commissioners should not forget general success in their scrutiny of minute details.

The Superannuation of Officers in British Hospitals for the Insane: Its Principle, Policy, and Practice. By W. LAUDER LINDSAY, M.D., F.R.S.E. London: 1875.

THOSE who are connected with lunatic asylums have much reason to thank Dr Lindsay for the great amount of information he has collected about the salaries and allowances of officials. It is often a subject of some difficulty what wages should be offered for any particular duty, and Dr Lindsay's extensive tables, which he has collected with so much trouble, will often be of great assistance to the superintendents of asylums. Dr Lindsay advocates the granting of liberal retiring allowances to all the officials, high and low, as a means of attracting and keeping good servants. Dr Lindsay has been always distinguished for the disinterestedness of his character, and the courage and independence with which he has upheld the dignity of his profession. He gives a true and striking sketch of the difficulties, trials, and worries, which surround the medical superintendent of an asylum.

Extrauterine Pregnancy: its Causes, Species, Pathological Anatomy, Clinical History, Diagnosis, Prognosis, and Treatment. By JOHN S. PARRY, M.D., etc., etc. London: Lewis: 1876.

SINCE the Edinburgh memoir of Dr Campbell, published in 1840, there has appeared no such comprehensive and valuable work on this subject as that to which we now call attention by Dr Parry of Philadelphia.

Since Campbell's time, our knowledge has greatly increased over the whole field, and almost all the novelty has been faithfully swept into Parry's learned net. But there has been an equally remarkable increase of experience in the treatment of this disease. Formerly, such cases were much left to nature. Now, we suppose few cases are anywhere met with that do not command a consultation as to operative interference; and we know that actually many cases have been and are successfully treated by the surgeon's knife.

There is a part of this subject, however, to which Parry makes no considerable contribution, but on which much light is wanted. This is to be got only by careful and very difficult anatomical investigation. We want exactness of knowledge regarding those cases rudely classified as abdominal or ventral, and shall only hint that probably many of them will turn out to be really tubal. This is the scientific side of the subject whose aggrandizement always is sure to help towards progress in practice.

Methodik der Physiologischen Experimente und Vivesectionen. Von E. CYON. Mit Atlas. Giessen: J. Ricker. St Petersburg: Carl Ricker: 1876.

EVERY practical physiologist must feel deeply indebted to Prof. Cyon of St Petersburg, for this most excellent and by far the best treatise on physiological methods which has yet appeared. The text is accompanied by a magnificent atlas containing fifty-four plates, of which more than the half are quite new. Special attention has been given to the anatomy of the dog, rabbit, and frog, as far as is required for the purposes of the physiologist. The methods for chemico-physiological investigations have been entirely omitted, as they are sufficiently treated of in books specially devoted to that subject.

This book is a great contrast to anything that has yet appeared upon this subject, not only in point of comprehensiveness and in minuteness of detail, but on account of its accuracy and the evident care which has been taken that the description of any experiment shall not have been merely transcribed, as is too often the case, but the author seems to have employed most of the methods himself, and describes them with such minuteness as can only be acquired from a thoroughly practical acquaintance with the subject. Every detail in the process of experimenting is carefully considered, how difficulties are to be overcome is indicated, what parts are to be avoided and what divided, are all pointed out in a masterly manner.

The first chapter is occupied with some general rules concerning experimentation. The object of physiological experiments is indicated, a short sketch of the graphic method given, and the question of vivisection touched upon, and the author is most particular in inculcating the use of anæsthetics where such a procedure is permissible. A special chapter is devoted to the methods for keeping up artificial respiration.

The second chapter treats of hæmodynamics. Special experiments on the passage of fluids through tubes, for class demonstration, are given. Thus Ludwig's arrangement for showing the difference of pressure at different points of a tube of uniform diameter deserves special notice. By slight modifications of this apparatus, all of which are figured, the chief factors concerned in the movements of fluids can be demonstrated. These factors are the pressure exerted upon the moving fluid, the rapidity with which it flows out, the resistance which it has to overcome, and the power setting the fluid in motion. The changes in form and place of the heart, and the effect of temperature on it, are then discussed. A special chapter is devoted to the means of measuring the blood-pressure, the various forms of manometers, and the method of using them, as well as of testing their accuracy, are treated very fully. Then follow chapters on manometers for the frog's heart; the methods of measuring the rapidity of the motion of the blood; of observing the pulse; and experiments on the cardiac ganglia, to which is added a very useful table of the poisons which act on the heart, their dose, seat, and duration of action. The classic experiments on the cardiac nerves, viz., the vagus, accelerans, and depressor, are described very fully. The details for the exposure of each of these are described step by step, there being also special directions given. Experiments on the vaso-motor nerves, and on the reflex action of sensory nerves on them; observations on the action of vaso-dilator nerves, and methods of observing the influence of the respiration on the circulation, close this chapter. The third chapter is devoted to the consideration of respiration. In the fourth, experiments on secretion and excretion are given, including those on the secretion of saliva, urine, and lymph; and the formation of gastric, intestinal, pancreatic, and biliary fistulæ. Two chapters are devoted to the general physiology of nerve and muscle; the apparatus in most common use being all fully described and figured, including the elements and arrangement of batteries, commutators, rheochord, rheostat, multiplicator, bussole; the measurement of electromotor forces by means of the compensator and various forms of induction apparatus, and how these last are to be graduated. Next follows the arrangement of experiments for observing the electromotor force of nerve and muscle, electrotonus and the negative variation; for the stimulation of nerve and muscle, and testing their irritability; for measuring the rapidity of transmission of excitements of a nerve, a specially good figure of Fick's pendulum myograph being given. We miss, however, in the figure, the semicircular arc to which the apparatus for giving a greater or less swing to the pendulum is attached. The concluding chapter is devoted to special nervous physiology, including the experiments on the brain, cerebral nerves, spinal cord, reflex movement, disturbances of motion, and operations on the sympathetic.

Specially useful is the bibliography given at the end of each chapter.

The woodcuts of the Atlas embrace figures of all the instruments employed in physiological operations, Ludwig's method of injecting under continuous pressure being figured. A splendid woodcut showing all the apparatus as used in the physiological laboratory of Leipzig, for obtaining a continuous kymographic curve, is given in Plate XIII. Even the latest experimental methods employed by Ludwig's scholars are all figured, including those of Kronecker, Ceradini, Bowditch, Luciani, Woroschiloff, Stirling, and Mosso. The second part of this book, embracing the methods of the physiology of the sensory organs and psychophysic, is promised in the course of this year. The term "Vivisection" employed in the title of this book seems to be quite unnecessary, appearing as it does to us that "Methods of Physiological Experiments" would have been amply sufficient, and would not have sounded so repulsive to over-sensitive English ears.

From the short sketch we have given of the nature, extent, and elaboration in detail of this work, every physiologist will look forward with great pleasure to the appearance of the second part.

Lectures on Nursing. By WILLIAM ROBERT SMITH, Resident Surgeon, Royal Hants County Hospital. London: J. and A. Churchill: 1875.

THE twelve excellent and somewhat innocent lectures which compose this neat little work, were doubtless listened to with much interest by the nurses to whom they were delivered. We doubt the wisdom or necessity of their publication.

Going over too much ground to be exhaustive, they still contain material not really of much use to a nurse, and omit details of almost indispensable importance. It is, however, easier to criticize than to write a book of this kind. This one, like many others of the same nature, deals in platitudes suitable for the infancy of the subject. The text-book for Nurses is still unwritten.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LV.—MEETING VI.

Wednesday, 5th April.—Dr GILLESPIE, *President, in the Chair.*

I. *Dr John Duncan,* in the absence of Mr Spence, showed a patient who had suffered from an ANEURISM INVOLVING THE INNOMINATE, FIRST PART OF THE SUBCLAVIAN, AS WELL AS PART OF THE ARCH OF THE AORTA. Before anything in the way of operation was attempted, rest was tried, and half-drachm doses of iodide of potash were given four times a day. This treatment had proved successful, so that there was now consolidation and absence of all pulsation.

II. *Dr Finlay* showed a specimen of a RARE FRACTURE, viz., between the condyles of the femur, without, however, involving either. It had been obtained from a patient whose leg had been amputated for a bad compound fracture higher up the limb, caused by a fall from a mast on the knee.

III. *Dr Bruce* then read a paper giving his experience of PUERPERAL FEVER, which will appear in a future number.

Dr Gillespie said the Society was much indebted to Dr Bruce for the account he had given of his cases. He would now be glad to hear the opinions of the members and visitors.

Dr Matthews Duncan concurred with Dr Bruce in his conclusions; and he only wished further to express his gratitude for the results of his long and large experience. He observed that in one case Dr Bruce doubted his own salubrity, because two days before this case he had attended a post-mortem. But the question was really insoluble; for no one could say whether the case might not have occurred without any such presumed source of infection. The whole thing was possibly only a mere coincidence. All the cases given were excellent illustrations of the accidental non-epidemic nature of puerperal fever. He had now practised for thirty years, and had seen nothing like an epidemic in that time. This was made certain by the statistical records since 1854. Nor had there been any epidemics before that, though they were constantly hearing about them. As Dr Macdonald, however, had said at a previous meeting, the epidemic theory explained awkward cases where blame might be attachable to the practitioner. The epidemic excused him. He had now no scruple in taking away that excuse.

Dr James Carmichael remarked that, although his experience had not been so extensive as that of Dr Bruce, yet in over 1200 labours which he had attended, some six or seven cases of puerperal fever had occurred. These were not epidemic, but happened singly and at separate times during a period of eleven years. During the progress of the cases, although attending other confinements, as well as surgical cases, and frequently making post-mortem examinations, no bad result had followed in way of causing infection. He thought Dr Bruce had done good service by bringing his experience before the Society.

Dr Gillespie remarked that when acting surgeon at the Infirmary, he took charge of two midwifery cases at about the same time for a friend. One of these took puerperal fever, while the other did not. He got such a fright, however, that he declined attending any such cases while holding the appointment of surgeon at the Infirmary. But from what he had heard to-night, his scruples were apparently groundless.

Dr Bruce thanked the members for their kind remarks.

IV. *Dr Church* then read a case of PNEUMOTHORAX, which will appear in a future number.

Dr Gillespie said the Society was much obliged to Dr Church.

Dr Grainger Stewart had no doubt that all the members of the Society present had listened with interest to Dr Church's account of this remarkable case. He had had opportunity of studying it after its admission to the Infirmary; and he considered it worthy of attention on several grounds. (1) Its mode of origin, sudden, apart from effort, or from serious pulmonary complaint. (2) The large amount of air which was effused in a short space of time. (3) The relief from imminent death afforded by the operation. (4) The fact that in the short time elapsing between the rupture and the operative interference the wound had become so agglutinated as no longer to permit of the passage of air. (5) That no irritation resulted, and the air left in the cavity was speedily absorbed. He congratulated Dr Church both upon the management of the case and the manner in which he had brought it before the Society.

Dr Ballantyne had listened with interest to the paper. He had recently seen a case in which there was rupture of the lung, and therefore admission of air into the pleural cavity. No treatment was adopted either time; aud the patient got well, without any pleurisy.

Dr Church thanked the members for their kind remarks. He had brought forward the case, as he believed it presented some unusual features of interest.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXV.—MEETING VII.

Wednesday, 8th March.—Professor SIMPSON, *President, in the Chair.*

I. *Dr Underhill* read a paper on the STRUCTURE OF THREE CERVICAL POLYPI, which will appear in a future number of this Journal.

Prof. Simpson thanked Dr Underhill for his paper. He thought the cases interesting both pathologically and clinically.

Dr Keiller remarked that the preparations which Dr Underhill had exhibited microscopically were evidence of what he had always thought and believed to be true, that the mucous variety of polypus was much more vascular than the fibrous, and that this amply accounted for the hæmorrhagic tendency of such cases.

II. In the absence of *Dr Matthews Duncan, Mr Jamieson* exhibited for him a RHINOCEPHALOUS CYCLOPEAN FŒTAL LAMB, premature, having been found twin with a well-formed lamb, slaughtered in the shambles. It was, on the whole, of healthy natural formation, except in its head. As yet, Dr Duncan had not dissected it in order to verify the occurrence of the anteriorly-united cerebral hemispheres. The eye and its cornea were of oval form, having the long diameter transversely. The iris was double, and the one was in contact with the other. The pupil in the middle of each iris was natural.

III. *Mr Jamieson* showed an UMBILICAL CORD, TIED WITH AN ELASTIC LIGATURE, which had successfully arrested the bleeding which an ordinary ligature had failed to stop.

IV. *Dr Bruce* showed a preparation of a FŒTUS WITH EXOMPHALOS AND SACRAL HYGROMA. The anterior abdominal wall was entirely deficient.

V. *Dr Keiller* showed a LARGE SOFT FIBROID TUMOUR OF THE UTERUS, which had occurred in the practice of Dr Watson of Stockton-on-Tees. The tumour, which weighed thirty-four pounds, had grown for sixteen years, and was during life considered to be ovarian. The patient, when she died, was forty years of age, and had not menstruated during the whole period of the growth of the tumour. The uterus was enormously enlarged and nodulated, the cervix considerably elongated, measuring six inches. Both ovaries were healthy. He had not examined the preparation further, but intended doing so, and would report to a future meeting.

VI. *Prof. Simpson* read a paper on the COMPLETE EVACUATION OF THE UTERUS AFTER ABORTION, which appears at page 961 of this Journal.

Dr James Carmichael thought the subject of Prof. Simpson's paper important because it was commonplace. He agreed with the principles of the treatment laid down, as being essentially the same as those taught by the late Sir James Simpson. He considered it most necessary to procure speedy and complete evacuation of the uterus in abortion. To this end, after digital manipulation, with the administration of ergot, had been resorted to without effect, nothing was more useful in producing speedy contraction and evacuation of uterine contents than a uterine plug, consisting of a sponge-tent, strip of bandage, or other similar substance, pushed into the cavity with a probe. In his experience, it was a more pleasant, easy, and effectual means of producing uterine contraction, and thereby stopping bleeding, and evacuating the contents of the organ, than the vaginal plug.

Dr Keiller thought the paper of practical importance. The great object in all cases should be to evacuate the uterine contents, otherwise dangerous results were liable to occur. The most serious cases he had seen were cases in which abortion had been criminally procured, and as to which he had been repeatedly consulted. No doubt the explanation of the special danger was, that in these cases the tissues and vessels were more healthy than in ordinary abortion, where there was usually some previously-existing disease or degeneration of the ovum. The most important remedies in hæmorrhage were ergotine and perchloride of iron. He wished to call the attention of Fellows to the great benefit he had found from using his left hand in terminating these cases. He had found it much more serviceable than the right. To press down the uterus from above, while investigating and treating such cases, was of the utmost consequence. In cases of difficulty, he frequently had recourse to the introduction of the whole hand into the vagina, which could be in many cases readily enough done, especially under the influence of chloroform; being thus able to reach the uterine cavity digitally and efficiently.

Dr Gordon had seen most severe bleeding occur even weeks after, from retention of portions of the ovum. He approved of the removal of the whole abortion if possible; but some cases did better when left alone, for the uterus to empty itself by its own efforts.

Dr Milne said he was much interested in the President's paper, which was a very important and practical one. He had dealt with many cases of abortion, and was aware of the great importance of a complete evacuation of the uterus. He approved of the method of pressing down the uterus from above in difficult cases, and even, if necessary, hooking it down by the volsellum. When the finger could not be introduced, it was better to insert a sponge-tent into the os, and wait; and very frequently, on its withdrawal some hours afterwards, the entire ovum came away. This was better than the vaginal tampon. While, as remarked, it was very important to ensure the entire expulsion of the ovum, he must observe that, owing to organized adhesion between the ovum and the uterus, he had sometimes been compelled to leave bits of the former behind, and, although in some cases flooding resulted, in others there was neither hæmorrhage nor septic poisoning.

Dr Macdonald commended the paper. He thought it useful if for no other reason than to dispel the popular belief that interference with the interior of the uterus was necessarily dangerous. He agreed with Dr Keiller in the propriety of introducing the whole hand into the vagina if necessary. In practice he had not required to drag down the uterus with a volsellum. In regard to plugging, he did not think it was so often necessary as some practitioners supposed.

Dr Bruce had sometimes found great difficulty in removing the ovum entire, and in such cases he thought there was less risk in allowing small shreds to remain than in persisting to attempt their removal, which was not always possible. He had not been troubled with hæmorrhage or other bad results from adopting this course.

Dr Rattray thought that by keeping the patient quiet, giving ergot, and injecting cold water, most cases of bleeding might be satisfactorily treated. He was always in the habit of bandaging the patient in abortion, as after delivery. He dreaded retention of parts of the placenta.

Dr Macrae was disappointed with the paper. He expected Dr Simpson would have gone into the subject more fully. He agreed in the main with what had been said throughout the discussion. He could not see, with Dr Rattray, the necessity for a binder in such cases. He lately had a case in which he failed to remove the placenta, and left the case to nature, with good results. He was not frightened to have a small portion of placenta for a time in the uterus. As to plugging, he thought it a most unpleasant operation, and not often required.

Dr Wilson had found severe bleeding not unfrequent. He had confidence in plugging. Had not required to use the volsellum.

Prof. Simpson then replied. He thanked Dr Macrae for his condemnatory remarks, as it gave him the opportunity of stating that he had wished the paper had been more complete, but, as it was, it was only intended to speak with regard to a particular point in the subject. He thought retention of portions of the placenta was always a source of danger; and he could not agree with those who disregarded this. Judging from the remarks of Fellows, he thought the use of the volsellum had been neglected, as with it you could control the uterus independently of the muscular efforts of the abdominal walls. He did not approve of the injection of perchloride of iron, as it was a mechanical, not a therapeutical effect which was intended to be produced.

Part Fourth.

PERISCOPE.

CIRCULATION IN THE CORONARY ARTERY.—Brücke, in his physiological notes regarding the coronary artery, has stated that "the capillaries of the walls of the heart are emptied to disappearance during the contraction of the ventricles." This, however, had not been proved experimentally; and Dr Ferd. Klug, of Budapest, has accordingly made an experimental investigation into the subject on the hearts of frogs and rabbits. The heart was ligatured during systole and diastole respectively, the blood coagulated by diluted sulphuric acid, and microscopic examination made. In the frog, it was found that in diastole the muscle is rich in blood, whilst during systole traces of blood were found only in the outermost layers of the wall. In rabbits, all the vessels were found full of blood during diastole, especially rich in blood were the capillaries near the apex, those of the base being relatively poorer. In diastole, blood was found in the superficial vessels, whilst those situated deeper showed scarcely a trace. Sections from the apex contained no blood. According to these experiments, the heart's contraction so compresses the capillaries, that the blood is driven from the walls of the organ, whilst during diastole the blood enters them freely.—*From the Centralblatt*, No. 8, 1876.

SALOMON. *Speedy Cure of Severe Acute Disease of the Spinal Cord by Application of the Actual Cautery.*—An unmarried woman, aged 24, previously healthy, was seized with paralytic symptoms, which, within a few days, involved the muscles of the extremities and trunk. Notwithstanding the paralysis of the muscles of the back of the neck, the diaphragm and the respiratory muscles of the thorax were unaffected. Along with the paralysis, there was extensive anæsthesia, which extended over the extremities, trunk, and neck, even the tongue being affected. Of the face, only the forehead and scalp remained free. Firm pressure over the vertebral column was felt, but not the application of the actual cautery, which was applied on both sides of the whole extent of the spine. Within two days a considerable change was perceptible, and within two days more the sensibility was normal, except that in the inferior extremities a certain hyperæsthesia had taken place. The movements were all re-established, but weak. Within fourteen days the recovery was complete; the functions of the rectum and bladder had been very slightly disturbed.

The author considers the case to be one of the so-called "acute progressive paralysis." Different from what is ordinarily observed were, in this case, the complete anæsthesia and the retention of power by the respiratory muscles, so that a suspicion of a hysterical form of the paralysis might well be entertained. The speedy improvement after application of the actual cautery is analogous to the case of Dr Levy, who suffered from the same disease, and was cured, as he himself relates, by the same remedy (*Centralblatt*, 1874, p. 171). In the present case, there was further of interest the freedom of movement retained by the pupil, although there was paralysis of the vaso-motor fibres of the cervical sympathetic.—*Translated from extract in the Centralblatt*, No. 8, 1876.

J. CASPARY. *On the Contagiousness of Hereditary Syphilis.*—Günzburg has lately maintained that hereditary syphilis is not contagious. To show that this is incorrect, the author publishes the following cases:—The healthy wife of a labourer, who had borne and suckled two healthy children, became wet-nurse to an illegitimate child. This child was eight days old, and had an eruption about the mouth and anus, syphilis being established. Whilst she suckled the child, a sore developed on the mamma, and following it there was a roseola syphilitica. A year later she gave birth to a syphilitic child. In a second case, the author observed a child which immediately after birth sickened with an eruption, and was treated for hereditary syphilis by mercurial baths. When seven months old, the child seemed healthy, and was given to a wet-nurse. The nurse soon observed that there were sores on the inner surface of the thigh of the child, and three months later a fissure formed on the left nipple of the woman, which assumed the character of the Hunterian chancre. All the appearances of constitutional syphilis presented themselves, and later the communication of the disease to the husband, and the woman's own child, was observed. Finally, in a third case, a woman who, as well as her husband and child, was healthy, was infected by a child on whom the author had previously diagnosed hereditary syphilis. The woman, whilst under the author's observation, suffered from hard chancre of the mamma, consequent swelling of the axillary glands, and soon afterwards a papulo-squamous syphilide. From these cases the author considers the contagiousness of hereditary syphilis proven, and the opposite view of Günzburg as erroneous and dangerous.—*From extract in Centralblatt,* No. 6, 1876.

CH. RICHET. *On Somnambulism Artificially Produced.—Journ. de l'Anat., etc.*, 1875.—The author experimented on about forty persons (amongst whom were two males), and in consideration of the number and concordance of the appearances, believes all suspicion of simulation to be excluded. He comes to the following conclusions:—(1.) By the so-called "magnetic passes" as well by gazing on a shining object, and by other empiric, but as yet little known and unreliable procedures, a condition of the nervous system analogous to somnambulism can be produced. (2.) This condition is excited the first time with difficulty, but almost always appears when the process is several times repeated. When it has been produced once it can be easily excited afterwards. (3.) All the phenomena which are then observed can be explained by known physiological and psychological facts, and are also observed in several kinds of intoxication (alcohol, chloroform, haschisch), and in several diseases of the nervous system (hysteria, epilepsy). (4.) Especially characteristic phenomena are hallucinations, which can be produced at pleasure, and a completely automatic condition, so that the person who has been put asleep is subject to the will of those around him, and perceives sensations of which they speak to him.—*From extract in the Centralblatt,* No. 6, 1876.

POUCET. *Note on the Comparative Weight of the Bones of the Superior Extremities; Application in Medical Jurisprudence.—Gaz. Hebdom.* 1875, No. 36.—The question if an individual was, during life, right or left handed, can be decided by comparison of the weight of the bones of the upper extremities. In 18 persons, known to be right-handed, there was an average difference of 17 grammes in favour of the right side; on the other hand, the bones of the left arm of a left-handed woman, thirty-two years old, weighed 17, and those of a left-handed girl seven years old, 3 grammes, more than those of the right arm. This difference is not present at birth, develops first after several years, is greatest in middle age, and later decreases. In one case these observations decided the question between murder or suicide.—*From extract in Centralblatt,* No. 6, 1876.

F. A. KEHRER. *Experiments on the Power of the Lochia to Produce Inflammation and Fever.—Beitr. z. vergleich. u. Exp. Geburtsh.—Giessen,* 1875.—In conjunction with Rokitansky junior, Kehrer found that subcutaneous injection of the lochia of healthy puerperal women produced in rabbits invariably inflammation and suppuration of the areolar tissue. The lochia taken some days later produced larger abscesses, which did not differ from those which followed putrid lochia of the earlier period. The lochia of the fourth to the sixth day killed the animals, which died with the appearances of inanition. Their temperature was always somewhat elevated, but was unsteady, and sank considerably before death. In inoculations on the thighs of the women themselves, the lochia of the third day always produced inflammation of the inoculated part. By this phlogistic property of the normal lochia is to be explained the ordinary conversion of fissures of the mucous membrane into ulcers, and the frequent surgical fever (Wundfieber) of puerperal women. The reason why they do not all fever is, that at the time when the lochia have acquired the phlogistic properties (on the second or third day) the wounds are already granulating—that is to say, are covered over. The practical conclusion to be drawn from these results is, that the lochia must in every case be considered dangerous; that, further, all interference subsequent to the delivery (examinations, removal of retained portions of the placenta) which might separate healing excoriations of the mucous membrane is to be avoided, and that wounds are to be healed as rapidly as possible.—*From extract in the Centralblatt,* No. 3, 1876.

A. Model. *Excision of a Migratory Gall-Stone.—Bayer, ärztl. Intelligenzbl.,* 1875, No. 41.—Model extracted from a woman, after widening an exceedingly small fistulous opening, a gall-stone of the size of a Muscat nut, consisting mostly of cholesterin. The stone lay in the abdominal wall, in a cavity nearly as large as a walnut, which was lined by a smooth membrane, like a serous membrane, and was enveloped in an abundant thready, pale yellow, and almost transparent mucus. No communication with the gall-bladder was discovered. The previously-existing severe pain was relieved by the operation, and after five weeks the fistula was closed.—*From extract in the Centralblatt,* No. 3, 1876.

Lube. *A Typhoid Epidemic Spread by Infected Milk. Allgem. Zeitshr. f. Epidemiol.,* 1875, S. 298.—In the small town of Plön there broke out suddenly, in August 1874, a slight epidemic of typhoid fever, twenty-four persons being affected by the disease. The first case of illness began on the 21st August, the last on the 26th September. From the beginning the cases were scattered over the whole town, although the elevation, subsoil, water, and drinking-water, are very different for different parts of it. Of the twenty-four affected persons, twenty-one had, according to the custom of the country, drunk new milk, the milk being from one dairy. At this dairy-yard a well adjoins a dunghill, and the water, when examined chemically and microscopically, was found to be impure from organic matter. This water was used in washing the milk utensils. (The author does not believe that it was used to dilute the milk.) The author is of opinion that from this well typhoid germs were conveyed to the milk. How they came to be present in the well was not discovered. However, four cases occurred at the dairy, but they began at the same time, or even later, than those which occurred in the town. After the use of the suspected milk had ceased (6th September), only three fresh cases occurred, one of which was that of a child to whom, from exceptional circumstances, some of the same milk had been given.—*From extract in the Centralblatt,* No. 10, 1876.

Professor Lang of Innsbruck has published a paper on the Histology of Lupus Tissue in the 2d Heft of the *Wien. Medizin. Jahrbücher* for 1875. He comes to the conclusion that lupus is a disease of the skin which is characterized by a continual production and degeneration of connective tissue, vascular and epithelial structures. According to the stage of the disease, the progressive or retrogressive processes, and the one or the other tissue becomes most prominent; but there is always to be made out that a cell-proliferation which takes its origin in the vessels plays the chief part, and that in the last stages not only are the degenerated products absorbed, but new connective tissue is formed by cell-organization, so that the lupus skin may acquire a cicatricial appearance without any preceding ulceration.

FIBROUS TUMOUR OF THE LOBE OF THE EAR.—During the last six years, Dr Knapp has met with seven or eight cases in his practice. They all sprang from the hole pierced for the wearing of ear-rings. They grew slowly after slight inflammatory irritation. The author sees a similarity in the tumours of the lobe to the larger fibroid tumours of the eyelids. It is said that these "fibromes" of the lobule of the ear occur more frequently amongst negresses than the white population; and this is accounted for by the poorer negresses wearing ear-rings made of an inferior and heavier material than their white rivals. According to Dr Knapp, these tumours project more posteriorly, they increase slowly and without pain, and are seldom larger than a hazel-nut. Dr Knapp relates a case where the tumour on the right ear reached the size of a hen's egg, whilst that of the left side was the size of a hazel-nut. The patient was a negress, twenty-two years of age, who had her ears pierced two years previously. The tumours were removed by the use of the knife. The microscopic examination of the tumours showed a fibrous framework with darker homogeneous interstices. A canal filled with softer fibrous textures and fluid of a darker shade marked the position in both tumours of the piercing for the ear-rings. From these canals a framework of fibres spread out in different directions. These consisted of a firm intercellular substance, whose fibrils lay closely together, and only few cells arose from them. The soft transparent parts surrounded with the framework of fibres contained greater masses of round, spindle, and star-shaped cells, which anastomosed with one another, and were separated from one another by a clearly homogeneous or finely-striped intercellular substance. Neither mucus nor cartilaginous structure was found in the tumour. This was therefore a pure "fibrome" (fibrous tumour of the lobule—Wilde), arising from the substance of the lobule of the ear, which is well known to be of a fibrous nature, quite different to the cartilaginous structure of the auricle itself.

Dr Knapp does not agree with other authors who state that these tumours speedily return after removal, but that if they return it is owing to part of the growth having been left.—(Dr H. Knapp, of New York, in Archiv. für Augen- und Ohrenheilkunde, Band v., 1st Abth., 1876.) This description of fibrous tumour of the auricle, in its origin, growth, structure, and nature, as given by Dr Knapp, minus the similarity to fibrous tumour of the eyelid, coincides with that of a paper read before the surgical department of the British Medical Association at last year's Edinburgh meeting.

THE HISTOLOGICAL CHANGES WHICH TAKE PLACE IN THE LABYRINTH FROM CERTAIN INFECTIOUS DISEASES.—Professor Moos gives the history, with the post-mortem appearances and histological changes of the labyrinth, of six cases of "ileotyphus," which during the fatal disease suffered from affection of the ear. In all the cases purulent inflammation of the middle ear along with, in the labyrinth, "an infiltration with lymphoid cells or small cell infiltration." The parts affected in all the cases were the utricle, the saccule, the ampullæ, and the lamina spiralis membranacea. Those only occasionally affected were the semicircular canals and the osseous zone. That these pathological conditions belong specially to typhus is still doubtful, as in most cases of purulent inflammation of the internal ear, dying whilst the disease is still active, much the same conditions have been found; and the author in the same work relates how that this "Rundzellen-infiltration" has been found in cases of purulent inflammation of the middle ear in fatal cases of scarlet fever. We incline more to what Gruber has stated in his "Lehrbuch der Ohrenheilkunde, 1870," that in most cases of acute inflammation of the middle ear the labyrinthine structures are affected in a similar manner. Dr Moos gives other two cases, where, after recovery from typhus, in one case total deafness of both ears, and in the other complete deafness of the left ear, resulted. Dr Moos has given the microscopic appearances of three forms of infectious diseases:—1. Changes of the labyrinth from ileotyphus; 2. Inflammation of the labyrinth from scarlet fever; 3. Purulent inflammation of the labyrinth from variola. This paper, worthy of the author, is the result of long-continued and careful seeking after results which appear to us as yet not to establish any specific affection of the labyrinth, from certain infectious diseases.—Professor Dr Moos, Heidelberg, *Archiv. für Augen- und Ohrenheilkunde*, Band v., 1st Abtheilhung. 1876.

TREATMENT OF MENIÈRE'S DISEASE WITH QUININE.—A case of Menière's disease, which had resisted all forms of treatment, came into the Salpétrière, under the care of Professor Charcot. The patient had been confined to bed for ten years, while the feeling of faintness was developed to such a painful extent, that the slightest movement of the bed, or the least movement that affected the bed, made the case desperate. The patient suffered, besides, from constant tinnitus in the left ear. From time to time, whilst the patient was perfectly quiet, the feeling occurred as if being thrown forward or backward—less seldom to one or the other side. Consciousness was always retained. A subjective noise in the ear preceded the feeling of being pitched forward or backward (this noise in the ear resembled the shrill whistle of an engine), and was often ended by a fit of vomiting. The observation which Charcot had several times made, that by the cessation of the noise in the ear the other complex symptoms of the disease also ceased, induced him to give large doses of the sulphate of quinine to substitute the noise in the ears of the quinine for that of the tinnitus. The patient took 0·75 to 1·20 grm. daily for nearly three months. Five weeks after beginning to take the quinine, the patient was able to be removed from one bed to the other. The tinnitus began to be superseded by the noise from the effect of the quinine; at the same time the feeling of faintness diminished the other symptoms also; and, finally, all the symptoms of Menière's disease left. The patient recovered so well, that by the assistance of a staff she could walk about, but she remained deaf as before.

Professor Charcot relates a second case of a lady who was cured by quinine of Menière's disease.—*Gazette des Hôpitaux*, 1875, Nos. 95, 98.

SOME most unfortunate personal disputes have arisen between some of our most eminent scientific men, and have widely engaged the attention of all our medical circles. The dispute which has created the greatest sensation is this:—Dr Krönlein, formerly the assistant of Professor Roser, the celebrated Zurich surgeon, had, while in that position, many opportunities of studying the advantages of the open treatment of wounds, and he has produced a very clever and elaborate treatise on this subject. After relinquishing his post at Zurich, he visited the surgical clinics of the various German Universities, in order to observe Lister's antiseptic treatment of wounds, which is now generally adopted in Northern Germany. The result of this tour, which included, amongst others, a visit to the surgical clinic at Halle, to which he was most kindly admitted by the director, Professor Volkmann, was a very careful and elaborate treatise, comparing the results of both methods. It is much to be regretted that in this clever work, in which he gives the decided preference to the open treatment of wounds, he has not entirely abstained from unnecessary personal attacks. Though specially acknowleging Professor Volkmann's kindness to him, he ventures to question the accuracy of his statistics, objecting to them on the ground that one case had been omitted, for the sole reason that the patient had succumbed under the treatment. Professor Volkmann was not slow in replying to this direct accusation of dishonesty; but, unfortunately, his reply is still more personal than the attack. The Professor does not confine himself to a scientific defence, but pours upon his opponent a torrent of personal invective: the concluding paragraph of his work is a fair sample of the whole:—"Whoever combines such ignorance with such want of truthfulness, as Dr Krönelin has done, ought to be kicked out." It is very sad when men of science descend to so low a tone as this in their intercourse. The end of the dispute was what might have been expected. Dr Krönlein does not take any notice of the unworthy attack of his opponent, but confines himself to refuting the scientific arguments of the Professor, observing, in conclusion, that the tone of his polemic was such that it would be impossible to enter again into any discussion with him.—*Berlin Correspondent of the Medical Examiner.*

MONTHLY REPORT ON THE PROGRESS OF THERAPEUTICS.

By W. Handsel Griffiths, Ph.D., L.R.C.P. Ed., Licentiate of the Royal College of Surgeons of Edinburgh; Lecturer on Medical Chemistry in the Ledwich School of Medicine, Dublin; Corresponding Member of the Therapeutical Society of Paris; Honorary Member of the Ontario and Chicago Colleges of Pharmacy, etc.; Librarian to the Royal College of Surgeons in Ireland.

[The author of these Reports will be glad to receive any books, pamphlets, or papers relating to Materia Medica or Therapeutics. They may be forwarded through the agencies of the *Edinburgh Medical Journal.*]

Ergot.—(*a.*) *Physiological Action.*—Dr P. Ebertz concludes from his investigations that the diminution in the frequency, and the increase in the fulness of the pulse, produced by ergot, is due to the direct action of the drug on the terminals of the vagus in the heart. The origin of the vagus is not affected, and the vagus itself is not paralyzed. The increased blood-pressure produced by ergotin is the result of the direct irritation by the drug of the vaso-motor centre.—*Schmidt's Jahrbucher,* clviii. p. 128.

The following are Wernich's conclusions:—In certain vascular tracts (skin, muscles, intestine, bladder, pia mater, and spinal cord), ergot produces contraction of individual arteries, especially when it is injected directly into the veins; and this action is not directly influenced by division of the sympathetic. Perhaps on account of the peculiar arrangement of the uterine vessels, contraction of these is not well marked. Contractions of the whole uterus occur after injection of ergot, but these are somewhat later than contractions of the vessels in other organs, but before the contraction of the uterine vessels—indicated by the uterus becoming paler. These contractions are probably caused by irritation of the brain (possibly anæmic), or of the motor centre of the uterus high up in the cord; for the contractions do not occur after severance of the cord.—*Virchow's Archiv,* lvi. p. 505.

(*b.*) *Effects of the Alcoholic and Aqueous Extracts of Ergot.*—Wigger's extract is an alcoholic extract, while Bonjean's is an aqueous extract. Dr H. Wohler finds that these are very dissimilar in their action. Bonjean's ergotin stimulates the depressor centres in the heart and the vaso-motor centres in the medulla oblongata, thereby producing slowness of pulse, contraction of arteries, and consequently increase of blood-pressure. Wigger's ergotin has no effect on the heart or arteries, but is an acrid poison. Both ergotins reduce temperature and retard respiration, but

Wigger's does so in a greater degree than Bonjean's. Bonjean's ergotin reduces the irritability of the peripheral motor nerves where it acts directly on them, while Wigger's increases it. Both dilate the pupil, and both diminish irritability of the sensory nerves. It would thus appear that Wigger's ergotin is useless as a hæmostatic, but, inasmuch as it increases the irritability of the peripheral motor nerves, it is well, in order to produce labour, to combine it with Bonjean's ergotin, or to give ergot itself in substance.—*Virchow's Archiv,* vol. ix. pl. 3.

In *Lo Sperimentale* for August and September 1875, Dr G. Levi, writing on the action of ergot, arrives at the following conclusions: —1. The therapeutic effects of ergot are due to the presence of phosphoric acid. 2. The diseases in which ergot proves an useful remedy are equally benefited by phosphoric acid. 3. Two distinct series of phenomena are produced by ergot: physiological (ergotism), by the vegetable principles (ergotin and ecbolin, etc.); obstetrical, by the phosphoric acid. 4. The hæmostatic and ecbolic effects are produced with equal intensity and readiness by phosphoric acid and by ergot. 5. The quantity of phosphoric acid found by analysis in recently powdered ergot, in old powder, and in the aqueo-alcoholic extract, is proportionate to the obstetric effects produced by these substances respectively.

(*c.*) *Ergot in Epistaxis.*—J. Lauber finds injections of extract of ergot to be very effectual in the treatment of epistaxis.—*Schmidt's Jahrbucher*, clx. p. 122.

(*d.*) *Ergot in the Treatment of Insanity.*—Dr Eward C. Mann, in the *New York Medical Record,* 26th June 1875, writes on the use of ergot in the treatment of insanity. The three varieties of insanity in which ergot has proved useful, are—recurrent mania, chronic mania with lucid intervals, and epileptic mania. In these it is nearly always successful in reducing excitement, in shortening the attacks, in widening the intervals between them, sometimes in preventing their recurrence altogether, and in warding off the dangerous stage of exhaustion, by which maniacal excitement is so often succeeded. Ergot operates by its influence on the non-striated muscular fibres, thereby exercising a controlling power over the calibre of the intracranial vessels. In the three forms of mania referred to, the lesion consists essentially in cerebral hyperæmia. There are present in each form increased arterial pulsation, flushing of the face, suffusion of the eyes, dryness of the mouth, and cephalalgia. These phenomena are dependent on functional and not organic changes in the brain. In epileptic mania, a combination of bromide of sodium with ergot will materially aid the action of the latter in widening the intervals between the fits, and in modifying the attacks when they occur. The bromide of sodium is preferable to bromide of potassium, as it is pleasanter to the taste and causes less constitutional disturbance, when given in large doses.

In the *Allgemein Zeitschrift für Psychiatrie*, Band xxxii. Heft. 2, Dr A. H. van Andel gives results of cases of mania which he has treated by injection of ergotin. Each injection consisted of one decigramme (about 1½ grain) of ergotin in half a gramme each of glycerine and rectified spirit. He has employed this method of treatment in many cases, to counteract hyperæmia of the nervous centres, and with most beneficial results.

(*d.*) *Ergot in the Treatment of Fibrous Tumours of the Uterus.*—In his recent address to the American Medical Association, Dr W. H. Byford spoke at some length on the treatment of fibrous tumours of the uterus by ergot. He has collected 101 cases so treated by various authors; of these, 22 are reported cured, and 21 entirely resisted the treatment. Dr Byford prefers a fresh aqueous solution of the solid extract prepared by Squibb. A quantity equal to from one to two drachms of ergot is injected once every day or every two days. The combined use of belladonna and of bromides and iodides is advantageous.

ESERINE IN ASTHENOPIA AND SENILE PRESBYOPIA.—Prof. Gubler states that the disturbances of vision which succeed acute and sometimes chronic diseases, and which are due to consecutive paralysis, characterized by asthenopia and debility of the muscles of the eye, may be treated successfully by Calabar bean. In asthenopia a few drops of solution of sulphate of eserine ($\frac{1}{500}$ to $\frac{1}{200}$) put into the eye, will render the vision quite distinct in an hour or two. In presbyopia also, Prof. Gubler has applied eserine with advantage. He has found eserine to be of extreme value in retarding the advance of presbyopia.—*Gaz. Hebd.*, 4th Feb. 1876.

GALLIC ACID IN ALBUMINURIA.—Dr J. T. Jamieson (*American Practitioner*) records two cases of albuminuria as a sequel to scarlet fever successfully treated by gallic acid. He gave teaspoonful doses of the saturated solution every two hours. He thinks that the gallic acid is carried unchanged to the congested and inflamed capillaries of the secernent portion of the kidney, acts as an astringent and tonic to them, and promotes their contraction, thus arresting the exudation of the red blood-corpuscles and promoting the normal secretion of urine. He has seen no bad effects arise from the free administration of the acid. It does not disturb the stomach or interfere with the appetite or digestion; but it does tend to produce constipation, so that the occasional use of a laxative is rendered necessary.

BROMIDE OF IRON IN CHOREA.—Prof. da Costa prefers bromide of iron to all other remedies in the treatment of chorea. He gives it in increasing doses, commencing with five grains for a child, and rapidly increasing to twenty grains. He states that it is also of much value in the treatment of incontinence of urine in children. Local chorea or clonic muscular spasms, as twitching of the eyelids, etc., in hysterical women, are often cured by bromide of iron, when other remedies have failed.—*New Remedies,* July 1875.

BROMIDE OF POTASSIUM IN EPILEPSY.—Dr Otto has had very favourable results in the treatment of epilepsy by bromide of potassium. Most of his patients easily bore 185 grains daily, but Dr Otto advises against exceeding 230 grains. In recent cases he gives about 150 grains daily, divided into four doses, adding a little opium if the salt produces diarrhœa. The long-continued administration of this dosage produced disturbance of the sensorium and motor paralysis, from which, however, the patients completely recovered on ceasing the administration of the drug. When given for two or three weeks, skin lesions were always manifested. These and the nervous lesions he regards as indications for discontinuing the drug or for diminishing the dose. Dr Otto believes that bromide of potassium acts in these cases by overcoming the irritability of the central ganglia and peripheral nerves.—*Archiv für Psychiatrie ; Memorabilien,* 8th April 1875.

IODIDE OF POTASSIUM.—(*a.*) *Action of.*—Prof. Binz believes that in a healthy stomach iodide of potassium becomes changed, in part, by the hydrochloric acid into chloride of potassium and hydriodic acid ($KI+HCl=KCl+HI$). Another portion of the iodide of potassium is acted on by the chloride of sodium, so that iodide of sodium is formed, and, if the dose be large enough, a third portion remains unaltered. All these combinations quickly pass into the circulation. The hydriodic acid then meets with soda, and forms iodide of sodium ; but the alkaline combinations of iodine are again acted on in the tissues by carbonic acid and other acids, and iodine is set free. This free iodine has the property of combining with certain albuminous bodies; and Binz believes that in this fact lies the explanation of the therapeutic action of preparations of iodine.—*Hay's American Journal*, July 1875.

Dr Laurie, of Glasgow, holds that the one distinct and indisputable action of iodide of potassium is that of stimulating the mucous membranes. He states that in cases of chronic hard dry cough, hours of suffering may be obviated by taking advantage of the powers of iodide of potassium to restore and promote the secretion of the bronchial mucous membrane, thus relieving the congested bloodvessels, and producing comparative tranquillity of breathing. When the secretion of mucus has set in, the salt should be used with caution, or its administration discontinued.—*Brit. Med. Jl.*, 2d Jan. 1875.

(*b.*) *In Syphilis.*—Dr Joseph R. Beck thinks that in syphilis mercurials merely mask the disease. His treatment chiefly consists in giving large increasing doses of iodide of potassium, combined with iron, Fowler's solution, and, if anæmia be present, quinine, valerian, and cod-liver oil. The doses of iodide of potassium are increased until iodism is established; the medicine is then discontinued for a week, and if, on recommencing the medicine, iodism is again induced, the patient is discharged as cured. The presence of iodism Dr Beck regards as sure evidence of perfect cure.—*Phil. Med. Times*, 13th March 1875.

JABORANDI.—(*a.*) *Physiological Action.*—Vulpian states that jaborandi increases most of the secretions, affecting first the salivary glands, then the lachrymal glands, then the cutaneous glands, next the glands of the mucous membranes, and lastly the liver, pancreas, mammæ, and kidneys. He concludes that it acts by paralyzing the inhibitory influence of the terminal branches of the sympathetic on the secreting cells.—*Progrès Médicale.*

(*b.*) *Action on the Heart, etc.*—In a valuable paper by Mr J. N. Langley, in the *Jl. of Anat. and Phys.* for October 1875, he discusses the manner in which jaborandi exerts a slowing or inhibition on the heart's action. He has invariably found that the ventricle stops beating before the auricles, although it not unfrequently happens that the auricles first show the effects of the drug. As a rule the auricles cease contracting before the sinus. Sulphate of atropia quickly restores the heart-beat, but with less and less readiness as the heart has been stopped for a longer time. The auricles show this effect most strikingly; and it would appear that the auricles have a greater tendency to functional activity than the ventricle. Their rhythmic beat is more difficult to stop, and when stopped is more easily set going again than that of the ventricle. According to Vulpian, jaborandi does not slow the heart's action if urari be previously given; he therefore concludes that the slowing is caused by stimulation of the peripheral ends of the pneumogastric inhibitory fibres on the heart. Mr Langley is led to believe, from a careful series of experiments, that jaborandi *does* slow the heart's action after urari has been given, and he concludes that it produces this slowing by acting on something else than on the inhibitory nerve-fibres going to the heart. Mr Langley found that an electric stimulus of definite strength sent into the pneumogastric of a toad, soon after the injection of jaborandi, when the heart's action is just a little slowed, produces a greater inhibitory effect on the heart than it does before the injection. In the rabbit he has not been able to determine any similar exaltation of the inhibitory function of the pneumogastric as the result of the jaborandi injections—a fact of which he does not offer any explanation. Singularly enough, this initial exalting influence ultimately gives way to exactly the contrary effect. Thus jaborandi, though it will produce its slowing effect even when the inhibitory fibres of the pneumogastric have been rendered inert by urari, first exalts and then later on apparently paralyzes these same inhibitory fibres. Mr Langley believes that a partial explanation of these contradictory results is to be sought for in the fact that the extract of jaborandi contains other active principles besides the alkaloid pilocarpine. For pilocarpine, although behaving in a general way exceedingly like the extract of jaborandi, differs from them in this particular action on the pneumogastrics. There seems to be present in the crude extracts of jaborandi—(1) pilocarpine, the effect of which in small doses is to paralyze the inhibitory fibres of the pneumogastric with a moderate slowing only, and in large doses to increase the slowing to a stand-still; (2) some other substance or substances which exalt the inhibitory function of the pneumogastric, and, at the same time, has a powerful action in slowing the heart. When the heart has been slowed or stopped by jaborandi, the injection of atropia brings back the beat almost to its normal standard. It is affirmed that, after atropia has been given, jaborandi will not produce a slowing of the heart. Recent experiments lead to the conclusion that a definite quantity of atropia cannot prevent a proportionate definite quantity of jaborandi from producing its effects on the heart. Mr Langley brings a very interesting paper to a conclusion, by discussing the bearing of the facts described on the theory of the action of jaborandi and atropia, and of the inhibitory mechanism of the heart. He supposes that these drugs act directly on the whole nervo-muscular cardiac tissue.

In a brief article in the *Edinburgh Medical Journal* for January 1876, Dr William Craig affirms that the strained infusion of jaborandi possesses all the activity of the drug. Belladonna and its active principle arrests the salivation produced by jaborandi; thus showing that, in certain respects, we have a true antagonism between jaborandi and belladonna.

(*c.*) *Mode of Action.*—The *modus operandi* is not attributable to any local action on the salivary glands during the act of swallowing. Dr Craig injected up the rectum the infusion prepared from one drachm of the bruised leaves, and found that salivation and perspiration came on as speedily and as effectually as when the same quantity was given by the mouth; clearly proving that the active principle of jaborandi is first absorbed, and afterwards produces its remarkable physiological effects. Dr Craig has never found jaborandi affect much either the heart's action or the temperature of the body, and he has failed to observe the disturbance of vision spoken of by Mr Martindale. He believes that many of the untoward results ascribed to jaborandi are due to the swallowing of the dregs instead of using a strained infusion.

(*d.*) *Therapeutical Application.*—Dr Craig has given jaborandi in several cases of fever, when the tongue was dry and the mouth parched. The drug has been found useful in removing pleuritic effusions; and several cases of diabetes have been successfully treated with jaborandi by Professor Laycock and others.

(*e.*) *In Diabetes.*—A very well-marked case of diabetes insipidus is recorded by Professor Sydney Ringer in the *British Medical Journal*, 25th December 1875, which was uninfluenced by jaborandi, while ergot, on three occasions, had the effect of reducing the quantity of urine.

(*f.*) *As a Galactagogue.*—In the *Medical Press*, 15th September 1875, Dr Ogilvie Will records a case in which jaborandi acted as a galactagogue. A decoction of two drachms and a half of jaborandi to six ounces of water was made, and of this a tablespoonful was given thrice daily, a strong decoction being at the same time applied to the breasts. After two doses milk appeared. The drug did not cause any marked disturbance of the sudorific or salivary systems.

(*g.*) *In Orchitis.*—Dr Czernicki records a case of orchitis consequent on metastasis of mumps, which he treated successfully with jaborandi.—*Gazette Hebd. de Med. et de Chir.*, No. 14.

Part Fifth.

MEDICAL NEWS.

PILL FOR THE KEIGHLEY BOARD OF GUARDIANS. — The *Anales de Ciencias Médicas* for 10th February last contains the following:—"Of the sixty-four individuals who died in Madrid from smallpox, during the past month, *no fewer than fifty-nine had never been vaccinated.*"

"UNAUTHORIZED VISITORS."—There is nothing more annoying than the constant ring of the door-bell; and it is still more vexatious when we find that in many cases such rings are utterly purposeless. Yet many persons find this nuisance is increasing. Formerly hawkers, old clothes' men, costermongers, and flower dealers were content to announce their presence in the street by stentorian shouts; now they invade the front door-step and area-gate. It is intensely unpleasant, in cold weather especially, to have the front door thrown open whilst the servant is vainly persuading the rough that neither brooms nor brushes, clothes' horses nor baskets, crockery nor ironware, are wanted; nor can one see an active barter going on from the area-steps of old bottles, rags, and dripping, without feeling that full bottles, new clothes, and joints of meat may likewise be disposed of. Another class also that give infinite trouble and no profit are the commission agents. These men, not content with the door-step, boldly force their way to our studies and waiting-rooms. Agents for publishing firms, with illustrated bibles or comprehensive cyclopædias; agents from wine merchants, with wonderful sherries, "free from acidity," often accompanied with cigars of "real Havannah;" men with furniture-polish, patent gas-burners, and photographs pirated from famous prints. These unauthorized visitors, independent of the annoyance they cause ourselves, entail much additional labour on the servants, who have to leave their work, and are kept constantly running backwards and forwards between the kitchen and the hall. It is our own fault we are pestered with commission agents, for unfortunately they have received hitherto too much encouragement, and we believe if they are henceforth treated as they deserve—that is, promptly shown the door the moment they disclose their purpose—our houses will not long be haunted by them. But the hawker, bottle merchant, and other sturdy ruffians are not so easily discouraged. The other day we saw a gang consisting of eight—men, boys, and women—selling brushes and other articles on both sides of Harley Street, ringing the house-bells, and making a hideous discord with their cries; nor did the different members of the gang hesitate, in spite of refusal, to ring the same house-bell over again. We have no wish to check honest industry, but we must protest against being annoyed by vagabonds of this class; and the police ought to be empowered to take in custody any person ringing a door-bell without a sufficient plea for doing so.—*Lancet, 5th Feb.*

OBITUARY.

Edmund A. Parkes, M.D., F.R.S.—Close upon the death of Dr Warburton Begbie, so eminent as a consulting physician, we have been deprived of another bright name, equally eminent in the different, but no less important, line of Military Hygiene. As cultivated and taught at Netley by Dr Parkes, that branch of science had assumed very large dimensions during the last fifteen years; and it would be almost impossible to over-estimate the amount of valuable, thorough, and clear-cut scientific work which he accomplished; the real improvements which he introduced into the clothing, feeding, and general comfort of our soldiers; and the enlarged views, the habits of accurate research, and the professional enthusiasm, which he was instrumental in conveying to the crowds of young surgeons who came under his influence.

This work, during half a generation at Netley, was the crowning flower, so to speak, of a reputation which had been steadily growing, under a variety of propitious circumstances. The roots were very vigorous and securely planted in the laboratory of his uncle, the late Dr Anthony Todd Thomson, where he learned habits of scientific research and familiarity with chemical manipulations. Three years spent in India, as an Assistant-Surgeon of the 84th, made him familiar with hepatitis, with dysentery, with cholera. Returning home in 1846, he wrote an important essay on the relations of Dysentery and Hepatitis, followed by one on Algide Cholera, and others on Diseases of the Heart. In 1849, when barely thirty, he was appointed, on the strength of his growing reputation, Professor of Clinical Medicine at University College, and Physician to the Hospital; still continuing active with his pen, he was chosen Gulstonian Lecturer at the Royal College of Physicians in 1855; and in the same year he was sent out by Government as an administrator in connexion with the exigencies of the Crimean War. He performed the duties and reported on them so admirably, that his capabilities were universally acknowledged; and after resuming his public and private occupations as a physician in London for five years, he felt constrained, by repeated illness of a serious nature, to leave the metropolis as his residence, and to accept the chair of Hygiene at the Army Medical School. This was in 1860. In 1864 he published an admirable work, the Manual of Practical Hygiene. This has reached a fourth edition, and has been translated into many Continental languages. Our space forbids any detail of the vast amount of scientific work which he accomplished during subsequent years. We may mention, however, (1) three papers, in the *Proceedings of the Royal Society*, on the Effects of Diet and Exercise on the Elimination of Nitrogen; (2) his experiments on the Effects of Alcohol; and a paper on the Effects of Brandy on the Temperature, Pulse, and Respirations of Healthy Men; (3) a Comparative Inquiry into the effects of Coffee, Extract of Meat, and Alcohol on Men Marching. These are but specimens.

Dr Parkes was a Member of the General Medical Council; a Member of the Senate of the University of London; and a Member of the Council of the Royal Society.

Those who knew him best agree in testifying that "valuable as was the work he did, better still was the way he did it: he was ever cheerful and bright, undismayed by difficulties, and equally unelated by success, but ever actuated by the purest desire to discover the truth."

The death of such a man at the age of fifty-seven is a great public loss, and the science of Hygiene, for which he did so much, has received in consequence a serious, although temporary, discouragement.

CORRESPONDENCE.

To the Editor of the Edinburgh Medical Journal.

DORPAT, 23*d March* 1876.

SIR,—In the March number of your Journal I met with an account of "Abeles' der Physiologische Zuckergehalt des Blutes," which article may give rise to the impression that the experiments of Dr Abeles refute those of my late friend Dr Bock and myself, published two years ago. This is the first time I find our work referred to in an English journal. Therefore, knowing very well with what interest the English medical public accepts every notice about diabetes, I beg you to be so good as to publish the following notice, not necessary in Germany, where every one may consult our work, but perhaps interesting to your readers after your notice about that new paper.

Bock and I have found by experimenting on the rabbit, that the sugar always met with in the blood of that animal disappears after shutting out the liver from the circulation, and ligaturing the aorta abdominalis and the ductus thoracicus close beneath the diaphragm. The time necessary for that purpose we found to be about eighty minutes. Many of our experiments tend to that result, seven (No. 67–74) seem to give the strictest evidence possible.

Dr Abeles made two experiments on fasting dogs. He excluded the liver out of the circulation, and ligatured the aorta. One dog lived thirty-six minutes, and the blood was found very rich in sugar; the other lived by artificial respiration forty-five minutes, and the blood contained as much sugar as the blood of other animals examined before.

By comparing these experiments, you see that they are by no means identical. Notwithstanding, it is very interesting that, forty-five minutes after the exclusion of the liver from the general circulation, no more sugar was found in the blood of the dog than the normal quantity. This seems a very good reason to believe that our experiments agree well with those of Abeles, and that, if the dog had survived the space of eighty minutes, his results would have been the same as ours. For after so great an operation as is necessary in shutting off the liver, the blood of the animal operated on must contain much more sugar than in the normal state; for this Pavy is the best authority. I made the same experiments on dogs one year ago, but these animals never lived so long as to permit the decision of the question.

Dr Abeles says himself, in his paper (p. 290), that his experiments cannot decide, and will, as he declares, repeat our experiments.

I hope he will, and it will be very agreeable to me, for I know too well that any one may make a mistake. I am myself now engaged in quite another way, and I have no hope of being able to revise the work here alluded to.—I am, etc.,

Dr T. H. Hoffmann,
Professor at the University of Dorpat.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Life of William Hunter:*[1] *The Harveian Address*, 13th April 1876. By J. MATTHEWS DUNCAN, M.D.

THE chief object of this annual meeting of medical practitioners, on the birthday of the immortal Harvey, is, by doing homage to the Prince of Physiologists, to testify our admiration and gratitude. We admire him as a scientific hero, whose name is indelibly engraved on the discovery of the circulation of the blood, and on embryological research. We are grateful to him, as being, in consequence of his grand discovery, by far the greatest improver of the noble art of medicine that has ever existed.

For us, medical practitioners, there is, as universal experience teaches, no guide so useful as the example of a great and good man who has passed before us; no incentive to high endeavours more powerful than applause or hope of fame. By celebrating the memory of Harvey we offer a guarantee to his followers, however great or however humble, that they also shall have their due place in the medical roll of glory.

In the London College of Physicians, to which Harvey belonged, an annual commemorative service is held in his honour, as is our custom also here. But between the practice of the two places there is this important difference; that, whereas with us each orator selects for the subject of his discourse any fitting matter—in fact, almost always, and most fortunately and appropriately, fixing on some great and famous physician or surgeon—the London orator repeats annually the praises of their great departed fellow and benefactor. It would be difficult to speak in too high terms of these repeated eulogies; and for this very reason is the annual task more onerous, and the merit of the achievement enhanced. The name of Harvey is admirably adapted for these yearly celebrations; and it may be doubted whether there be in the annals of medicine, from Hippocrates to our day inclusive, any other that would fill the great place. Certainly there is none that has nearly equal title to such high consideration.

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[1] The portrait is a photo-lithograph by Waterston and Son, from an engraving lent me by Dr Tuke. The autograph is an addition made by the lithographer from a class certificate. Through Sir W. Stirling-Maxwell, Mr George Waterston, jun., has ascertained that the original of this engraving is in the Print-room of the British Museum, the oval being enclosed in a broad border, not given in the reproduction from which the lithograph is taken. The inscription on the original is—"William Hunter, M.D.—J. Thornthwaite del. et sculp. 1780. Published as the Act directs, Feb. 20, 1780, by J. Thornthwaite, No. 390 Oxford Street." A medallion portrait is in existence, as well as several engravings, and two oil-paintings by Reynolds, who, as well as Hogarth, was his friend.

Your annual President, having wide scope of selection, experiences no difficulty in choosing from our copious and glorious medical ancestry, some one who presents admirable traits for pointing his moral and adorning his tale, some one whose special pursuits even may have been so like his own as to render his essay to address you a pleasant and congenial work.

I have fixed upon William Hunter; and, in the short time laid apart by you for this address, I shall occupy myself chiefly with matters and reflections such as may be truly said merely to go the length of fully justifying my choice. I shall dwell on the history of his life, on the nature and aim of his activity, and on its enduring products.

Before commencing, I may be permitted a digression, in order to show what William Hunter thought of Harvey; and this cannot be done without elucidating some special points in Hunter's character.

You will be astonished when I tell you on this occasion that, solemnly addressing his anatomical class, he made the following remarks in an introductory lecture:—"In merit, Harvey's rank must be very low indeed. So much had been discovered by others, that little more was left for him to do than to dress it up into a system; and that, every judge in such matters will allow, required no extraordinary talents. Yet, easy as it was, it made him immortal. But none of his writings show him to have been a man of uncommon abilities. It were easy to quote many passages which bring him nearly to a level with the rest of mankind. He lived almost thirty years after Asellius published the Lacteals, yet, to the last, seemed most inclined to think that no such vessels existed. Thirty hours at any time should have been sufficient to remove all his doubts. But this subject," he adds, "taken up in self-defence, grows unpleasant."

Such are the words of Hunter, and in order to avoid interference with his meaning, the whole paragraph is here quoted. I may at once say that they are, in my opinion, ungenerous and unjust. They are the result of a certain combativeness in my hero which led him, on this as on other occasions, to express his meaning imperfectly, in order to give greater force to his criticism. Hunter appreciated Harvey most justly and most highly, and he knew well, as his writings show, how much preparation is made previously by others for a crowning discovery, and how easy a matter the discovery seems to the ingenuous onlooker to have been—after it is made. How much did Kepler and others do for Newton? how much Mayer and others for Joule? William Hunter was frequently in controversy, frequently in self-defence. If we believe his testimony, all great anatomists have had great quarrels, and in the introductory lecture to his anatomical prelections, from which I have quoted, he is defending his views as to Harvey. The only proper course for him was to drop out of his lecture the offending and unjust paragraph, and to have left his other evidently more careful statements as they stood. "The authors," says Hunter, "of the three great discoveries in later times, and all the circumstances, are well known; I mean, the discovery of the western hemisphere by Columbus; of the true solar system by Copernicus; and of the circulation by Harvey. All these three men have equally acquired immortality; but they have not had, nor deserved, an equal degree of honour and credit." Harvey's discovery, he says, "was by far the most important step that has been made in the knowledge of animal bodies, in any age." Elsewhere he says,—"Dr Harvey, as appears by his writings, was certainly a first-rate genius for sagacity and application: and his name is deservedly immortal, on account of the many observations and improvements he made in Anatomy and Physiology." Having given these extracts, I pass to the more immediate subject of my address, feeling confident I have satisfied you that, had William Hunter lived among us, he would have been a good Harveian[1] and a promoter of our festival in honour of the great discoverer, to whom he had, in paramount respects, a strong resemblance as a philosopher and as an investigator.

It has been often remarked that persons of dissimilar character, who have similar aims, live together more amicably than persons of nearly the same character; and this is well illustrated in the life-long friendship of Cullen and William Hunter. At first, and for only a short time, conjoined in a sort of nascent partnership in practice at Hamilton, they were ever afterwards attached friends; and many letters of Cullen's to his "dear Willie," as he calls him, have been published. These two men form admirable illustrations of the two widely separated intellectual classes of great medical men. To the one class belong Hippocrates and Galen, and Sydenham and Cullen: they may be called the philosophers, those who exaggerate the powers and applicability of mere Reason; and they flourish still among us. The other class has a history beginning in Northern Italy at the revival of learning. "It was at this happy time," says William Hunter, "that Malpighi came forth, the great period for the study of all natural things. At this time the Academy del Cimento arose in Italy, the Royal Society in London, and the Royal Academy in Paris. And from that time, the important doctrine of rejecting all hypotheses, or general knowledge, till a sufficient number of facts shall have been ascertained, by careful observation and judicious experiments, has been every day growing into more credit. That doctrine was the source of

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[1] As further evidence, a class certificate of W. Hunter's was exhibited. It is of large size, and more than half of it is occupied by an engraved bust-portrait of Harvey.

Sir Isaac Newton's, and of all the improvements which have been made since the middle of the seventeenth century." This is the doctrine of the second great intellectual class of physicians, and to it belong Harvey, Morgagni, William Hunter, John Hunter, Laennec, and Charles Bell; those who give mere Reason its due subsidiary place, yet full scope. At one time the hostility between these classes was avowed, and strenuous efforts were made to smother the modern school of observation and experiment. In some of the great universities, graduates were required to swear to defend the doctrines of Hippocrates, Aristotle, and Galen, and not to permit their conclusions to be gainsayed by any person. This hostility is no longer avowed; and fortunately such a spirit of freedom prevails in scientific circles that its avowal would be certain to raise for it and its cause a legion of enemies, not of friends; but one can perceive the evident spirit of the two camps still everywhere prevalent, and no better example of it could be given than the quarrel between Broussais and Laennec. The great Cullen was of the philosophical school. His name is perhaps the greatest in it in recent times, and I shall here give only one note of this kind, by remarking that a great philosopher, favourably reviewing his works, points out that he probably did not add a single new fact to medical science.[1] William Hunter was of the opposite, or at least of a different kind. He was of the scientific, not of the philosophical class. Cullen's contributions to medicine, valuable though they are, might be dispensed with. The progress of medicine would be little disturbed by their loss. It is quite otherwise with William Hunter's. Without his contributions, as without Harvey's, progress is impossible. The honoured graves of Hippocrates and Cullen and their truly valuable works may be lost even to memory without very grievous results. The works of Harvey and of William Hunter and of this kind are indispensably necessary for progress. They cannot be forgotten, far less lost. Medical practice must wait till they are produced before it can make a real step in advance. Ingenious medical theories are extremely useful, but they do little for the advancement of medicine: often they retard it. The search for specifics or cures, and such pseudo-philosophical pursuits, have been continued for a thousand years, and as yet not one specific has been discovered. Of the spirit of William Hunter I shall give you illustrations from his own words, and if you read them in the light of the progress of medicine since his day, you will observe how true he was, not only as an apostle of scientific progress, but also as a prophet. "Were I to guess," says he, "at the most probable future improvements in physic, I should say that they would arise from a more general and more accurate examination of diseases after death. And were I to place a man of proper talents in the most direct road for becoming truly great in his profession, I would choose a good practical Anatomist, and put him into a large hospital to attend the sick, and dissect the dead." In another place he remarks as follows,—"In our branch (Anatomy), those teachers who take but little pains to demonstrate the parts of the body with precision and clearness, but study to captivate young minds with ingenious speculation, will not bear a reputation that will outlive them half a century. When they cease from their labours, their labours are buried along with them. There never was a man, perhaps, more followed and admired in Physiology than Boerhaave. I remember the veneration he was held in; and now, in the space of forty years, his Physiology is—it shocks me to think in what a light it appears." Still another extract to show his notion of how physic is to progress. "Anatomy is the art," says he, "of examining animal bodies by dissection. It teaches the structure and functions of those bodies, and shows nearly on what life and health depend. When these are well understood, a great step is made towards the knowledge and cure of diseases." Speaking of his researches on the anatomy of pregnancy, he truly asserts that they are of the utmost consequence "in explaining the phenomena of births and abortions, as well as in regulating our practice." "The more," he says, "we know of our fabrick, the more reason we have to believe, that if our senses were more acute, and our judgment more enlarged, we should be able to trace many springs of life which are now hidden from us; by the same sagacity we should discover the true causes and nature of diseases; and thereby be enabled to restore the health of many, who are now, from our more confined knowledge, said to labour under incurable disorders. By such an intimate acquaintance with the economy of our bodies, we should discover even the seeds of diseases; and destroy them, before they had taken root in the constitution."

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[1] See Sir W. Hamilton's "Discussions on Philosophy," etc.

Pursuing the same subject, Mr Babington remarks that,—"When at the close of the fifteenth century, medicine, in common with the other sciences, experienced a revival, it was the cultivation of anatomy, and not any closer attention to the symptoms of disease, which restored it to life. Vesalius was undoubtedly considered by his contemporaries as wasting, in the gratification of an idle curiosity, energies which would have been more usefully employed in the study of disease. Yet, while those who held this language are forgotten, Vesalius is justly celebrated as the founder of modern medicine. He first laid a solid foundation, on which the science has been erected by others. When, a century later, Harvey drew from anatomical and physiological investigations the discovery of the circulation, it is recorded that his fame as a physician was injured, because his talents were supposed to be devoted to pursuits foreign to the science of medicine. Yet this physiological discovery is now universally confessed to have first introduced the true knowledge of disease, and the true principles of treatment. In the same manner John Hunter was considered by the majority of his contemporaries as a theorist; as one who was rather a physiologist than a surgeon, and whose pursuits had little connexion with the practical improvement of his profession. Yet the works of the greatest surgeons of the day—of Cheselden, of Sharpe, or of Pott—were trivial and transient, when compared with the vast and enduring results which have proceeded from the theories of Hunter." These truths, as stated by Babington, I might illustrate by the researches and criticisms of our own day. But I avoid this tender topic, and conclude by reminding you of one item of the praise of Cullen and of John Hunter by the respected biographer of the former; namely, that Cullen did not introduce a new remedy for a disease, and Hunter did not invent a new instrument.

Although I have no intention of giving you a life of my hero, I wish to state some of the principal particulars of it. Numerous details are to be got from various sources, chiefly from a biographical account by Dr Samuel Foart Simmons, but also in Thomson's Life of Cullen, in Sir Benjamin Brodie's Hunterian Oration, in Gulliver's Account of Hewson, and other publications, such as the various histories of William's great brother John.

William Hunter was descended from the Hunters of Hunterston. His father had a small estate called Long Calderwood, in the parish of Kilbride, in the county of Lanark. There were ten children born to his parents. Of these William was the seventh, and John the youngest. William was born on 23d May 1718. He went to Glasgow College at fourteen years of age, and studied there five years with a view to enter the clerical profession. But his tastes did not lie this way.

In 1737, at nineteen years of age, he went to reside with Dr Cullen, who had just begun practice in Hamilton. After three happy years with this master, he resolved to go to Edinburgh and London, to prosecute medical studies, with the intention of subsequently joining Dr Cullen at Hamilton, as a partner in the business.

In 1740 he went to Edinburgh, and there he attended the lectures of Alexander Monro.

In 1741, when æt. 23, he went to London, and, first of all, resided with Dr Smellie, but soon entered the family of the well-known anatomist Dr James Douglas, as a medical pupil or assistant, and as preceptor to his son. He consulted his father regarding this step, and the old gentleman expressed himself as doubtful of its prudence, and wrote strongly in favour of his adhering to his intention to return to Hamilton to be Dr Cullen's partner, "where," he says, "you may be very comfortably settled and make money; and if you miss the opportunity now, you cannot be sure of it at another time."

About this time he became a surgeon's pupil at St George's Hospital, under Mr James Walker, and a dissecting pupil under Dr Frank Nicholls. This gentleman was a Reader on Anatomy, appointed by the Royal College of Surgeons. He gave his course in from thirty to forty lectures, and used two dead bodies during it.

Hunter's patron, Douglas, died in 1742, but Hunter continued to reside with the family.

In 1743, he communicated to the Royal Society a paper on the Articular Cartilages, which has been highly praised by Brodie.

In 1746, he was fairly launched in his career for life, succeeding Mr Sharpe as Lecturer on Anatomy, in an apartment in Covent Garden, to a Society of Navy Surgeons. He got seventy guineas as fees after his first introductory lecture, and, carrying the money home with pride, told his friend Mr Watson that it was a larger sum than he had ever before been master of.

In 1747, he became a Member of the Corporation of Surgeons. In the same year he went to Leyden, made the acquaintance of Albinus there, and returned home by Paris. In 1748, he was elected Surgeon Man-midwife to the Middlesex Hospital, and in 1749 to the British Lying-in Hospital. He had probably picked up some of the midwifery practice of his late master, Douglas; and his prosperity as an accoucheur was rapid and great. Fortunately for him, the two chief contemporary practitioners in this branch of medicine left the field and made an opening for him. Sir Richard Manningham died, and Dr Sandys retired. At this time he was joined in London by his "uncouth" brother John, who had probably been partly bred a cabinetmaker with his brother-in-law.

In 1750 he got the degree of M.D. from the University of Glasgow, and about the same time he began to practise as a physician[1] and give up surgery. He also now left Mrs Douglas's house, and went to reside in Jermyn Street. In 1751 he revisited his native place, showed particular interest in his paternal estate of Long Calderwood, gave directions for the repair of the house, and for the purchase of any lands adjoining that might come into the market. Riding within sight of the place one day with Cullen, the latter pointed it out to his companion, and remarked its conspicuousness; to which Hunter rejoined, "Well, if I live, I shall make it still more conspicuous." This was the only holiday, or the last holiday, he ever took.

In 1756, he was admitted a Fellow of the Royal College of Physicians.

About this time he had his great quarrel with Monro concerning the anatomy of the lacteals and of the testes; and some time after this dispute came another with his brother, concerning the anatomy of the placenta.

In 1759, Hewson began to assist him in his anatomical lectures, a connexion which ripened into a lecturing partnership in 1762, and which was ultimately and not quite amicably dissolved in 1770. Cruikshank succeeded Hewson. In 1762, he went to lecture in Litchfield Street, and in 1769 he went to the famous locality in Windmill Street.

In 1764, he was appointed Physician Extraordinary to the Queen.

In 1767, he was elected a Fellow of the Royal Society.

In 1768, he joined the Society of Antiquaries, and in the same year was made first Professor of Anatomy in the new Royal Academy of Arts.

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[1] Nowadays, we would say as an "obstetric physician" or "gynæcologist."

In 1774, appeared his great work on the Gravid Uterus, which he had begun twenty-four years previously.

In 1781, he was elected President of the Royal Academy of Arts.

William Hunter began, when about fifty-five years of age, to suffer from wandering gout. The disease gradually gained ground on him. Headache and squeamishness made him take to bed on the 15th March 1783. On the 20th, however, he rose to give his introductory lecture. At the close of it he fainted, and was carried to his bed again. He died on the 30th, aged sixty-five years.

On the 5th of April he was buried in the Rector's Vault of St James's Church, Westminster.

"His person," says Adams, "though small, was graceful; his cast of features regular and interesting; his voice musical; his manners attentive and flattering.[1] In short, Dr Hunter was a polite scholar, an accomplished gentleman, a complete anatomist, and probably the most perfect demonstrator, as well as lecturer, the world had ever seen." He was never married. His brother John's son and daughter died without issue, but the family is not extinct even in this branch; for there is at present in Scotland one medical man belonging to it, its sole medical representative. This is William Hunter, the present eminent practitioner in Rothesay, the great-great-grandson of Archibald, the only paternal uncle of Hunter, or whose great-grandfather was the cousin-german of the Hunters.

The estate of Long Calderwood is, I am told, now possessed by William Hunter Baillie, the son of the eminent Matthew Baillie, who was the son of the great Hunter's sister. Through this sister's marriage and descendants the Hunters became further connected with the well-known London medical names of Croft, Denman, and Brodie.

William Hunter's museum is said to have cost him £100,000, and he bequeathed a sum of £8000 to maintain it. His collection of coins cost him £20,000, and an illustrated work descriptive of it has been published by Mr Combe. He purchased for large sums the museums and literary collections of Sandys, of Fothergill, and of Hewson. The Rev. Dr Harwood describes his classical library as "the most magnificent treasure of Greek and Latin books that has been accumulated by any person now living, since the days of Mead."

William Hunter is generally known in the profession as a great obstetrician. This he no doubt was; certainly one of the very greatest that has ever flourished in this country or in any other. He taught midwifery,[2] and MS. notes of his excellent lectures are to be found in many libraries; and in that of our College of

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[1] There were exhibited a mask of Hunter's face taken after death; two portraits; his armorial bearings; and a class certificate bearing his autograph.

[2] Probably his special courses of lectures on midwifery were like Smellie's: twenty-eight different courses in twelve months, with from three to five pupils attending each. See the introduction to Smellie's works.

Physicians, there are two distinct and pretty complete volumes of such notes. His celebrity as a teacher of this branch was and is very great, but we know little of his lectures with sufficient authenticity. He published none of them, except those on obstetrical anatomy, and these partially only. His views, and even his words, are frequently quoted, and in my opinion without due caution; for none of us who are teachers would confide the accurate statement of our sayings to tradition through the pens of students. There can be no doubt, however, that he is justly classed by historians with Böer and with his connexion Denman, who revolted against the polypharmacy and otherwise meddling practice of the physicians of the day. He knew more of nature and of its powers, and reverenced it and trusted to it more than his contemporaries; he is indeed generally credited with teaching his class that the forceps had been a curse rather than a blessing to lying-in women, had done more harm than good. He wrote a few obstetrical papers of no great importance, and he published his grand work on Obstetrical Anatomy, of which I must speak again at length. It is this work that keeps his name constantly before the profession as a founder of the science of obstetrics. Yet, after all, the strength of his literary, scientific, and teaching power was not given to midwifery.

William Hunter was a great practitioner of midwifery. When he left Hamilton and his master Cullen to improve himself by a visit to London, he probably left behind him a country midwifery practice. In London he is said to have resided first with Smellie, a Scotchman from Lanark, recently settled there, an enthusiast in the science and practice of midwifery, a genius of obstetrics, whose works are even now recognised to have such value as to demand republication at the hands of the New Sydenham Society, which has secured for the congenial service of editor the great Dublin accoucheur Dr M'Clintock, a gentleman who is known to be also engaged in writing his biography. It must be surely in Smellie's house that William Hunter received the bias to midwifery practice that he afterwards showed. Smellie never had a fashionable connexion, and in this respect Hunter far surpassed him, being not only fashionable, but enjoying the distinction of accoucheur to the Queen of the reigning Sovereign, a sufficient passport everywhere to distinguished and lucrative employment. It has been often said that there was ill feeling or jealousy between the two Scotchmen; but while there is not sufficient or indeed any good proof of this, it is very remarkable that we find little evidence of any kind to show that they had even frequent intercourse one with another.[1]

This conjunction of obstetrical practice with anatomical teaching and study, is well worthy of consideration;[2] and, did time permit, would suggest many reflections arising from the circumstance that it would not be tolerated in our time.

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[1] That they met, and, at least once, held consultation we learn from Smellie's works. See *Collection* xx. No. ii., Case 1; also *Collection* xxxvi. Case 4.

[2] The great Harvey also conjoined the teaching of anatomy and the practice of midwifery.

We know, then, that William Hunter was a fashionable practitioner and a great author in obstetrics; that as a practitioner his methods were simple, and characterized by great reliance on nature; that, as an author, his chief obstetrical work was anatomical. William Hunter, however, did not give his great and sustained powers to midwifery, but to anatomy. His abilities, his zeal, his great fortune, were devoted to this. To use his own phrase, he made a great figure in anatomy. We know from his own mouth that it was as an anatomist that he claimed eminence and expected fame; and although his reputation has latterly become too exclusively obstetrical, it has not even in this respect been too highly exalted. Yet it is necessary, with a view to justice, to point out that his obstetrical fame is chiefly anatomical, and that his greatest claim on our admiration and gratitude arises from his anatomical work and influence.

William Hunter may, indeed, be said to be in many respects the founder of the modern London School of Medicine as a good place of education. There were, indeed, teachers before him, as his respected master, James Douglas, another Scotchman, in whose family he resided in his early days, whose children he had charge of as tutor, of whom he speaks with affection, and whom he assisted in a projected work of plates of the bones. But William Hunter first gave a systematic, complete, illustrated course. "Some people," says he, "thought even my former courses too long. Why? They had been used to see a course of anatomy finished in thirty or forty lectures, and therefore imagined that when it took up four months it must be unnecessarily minute or prolix." . . . "It has been objected, likewise, that a lecture of two hours' continuance is too much: the attention must flag, and the memory cannot carry the substance of it away. My answer," says he, "to this is, that if there be useful business enough for two hours a day through the proper season, so much time nearly must be given up to each lecture, otherwise a number of material things must be omitted. And," he adds, "there is enough of useful matter."

William Hunter was Professor of Anatomy in the Royal Academy. His enthusiasm as an anatomist knew no bounds. Anatomy was the passion of his life; and it was strong in death, for while just dying he expressed a wish that he could once more return to his anatomical theatre, to realize the happiness he had enjoyed there. "I wish now," said the dying man, "that I had but strength to bear being carried into my theatre, that I might tell my pupils how much comfort and happiness I feel." "Anatomy is," he says, "the only solid foundation of medicine. It is to the physician and surgeon what geometry is to the astronomer. It discovers and ascertains truth, overturns superstition and vulgar error, and checks the enthusiasm of theorists and of sects in medicine, to whom perhaps more of the human species have fallen a sacrifice than to the sword itself, or to pestilence." Speaking of the improvement of anatomical teaching in London, he said it was "owing to one," meaning himself, "who, with very moderate abilities, happened to have an uncommon love for the study, and who therefore took uncommon pains both to inform himself and inform others. Hence it is that London has been for some years one of the best schools for anatomy; and hence the London teacher is become possessed of a collection both of preparations and books inferior perhaps to none in Europe. He wishes to teach anatomy to the best advantage of his pupils while he enjoys life and health, and to perpetuate the spirit for anatomy in this country as far as human institutions can receive perpetuity." Latterly he lectured for six months in winter, and declined lucrative employment, to give him more time for his teaching, thinking "he could do infinitely more good to the public by teaching his art than by practising it." This noble and enlightened zeal led him to spend, it is said, about £100,000 on his museum; and at the close of his life he made a vain attempt to secure the royal patronage and assistance for his plan of founding an anatomical school or school of medicine at his own expense, offering to lay out £7000 from his own pocket, and intending to give in addition his museum and labour.

But good-will and zeal and liberality are not sufficient to complete William Hunter's title to fame as an anatomist. Let us, then, consider what there is of a more substantial kind to found this upon.

What were the great things that William Hunter effected during his life which make him much more noteworthy than most other medical men of his day? There are great things done in medicine which are not, at least not easily, shown to have any special connexion with the time at which they were made known; neither demanded by the time, nor the natural product of the time. They appear suddenly, abruptly, and remain isolated in science. Of such are the introduction of vaccination and of artificial anæsthesia. Other great things are done in medicine which are not indeed called for by the time of their appearance, but which are in many senses its product, and which have great influence. Of such are the great philosophical or speculative systems. The great Cullen's chief work was in this class. But the greatest thing in medicine is that scientific work which, like Newton's in physics, may in many senses be said to be demanded by the time and to be the product of the time. Of such was the discovery of Harvey. William Hunter's work was in this category. His was the great mind which saw what was most wanted in medicine, what was demanded, what the time was ready to produce if properly urged by a fitting genius.

When William Hunter settled in London, medicine, and especially medical education, was in a wretchedly imperfect condition, not flourishing. The education of a student consisted of an apprenticeship, a course of lectures on an tomy, and a few months of walking an hospital. The course of lectures varied from twenty-three to thirty, or perhaps forty, prelections of an hour each. The number of bodies used in dissection was, in a course which Hunter attended, only two. This scanty supply was eked out by using dogs for some of the demonstrations. Students never, or rarely, dissected parts, as at present. It is difficult for us now to conceive the importance of the course of anatomical lectures. It was all that most students had, on not anatomy merely, but physiology, medicine, surgery, and midwifery. William Hunter at this juncture devoted himself to teaching with all his powers, stimulated by all his heart. He expanded the course to four months, giving daily (and not omitting Saturdays) a two hours' lecture, forming thus a course much surpassing our longest courses at present. He founded the Windmill Street School. He was an excellent lecturer, and we have ample testimony that he carried out his purpose expressed in his own words, as follows:—"I . . . shall never aim at showing what I know, but labour to show, and describe as clearly as possible, what the students ought to know. This plan rejects all declamation, all parade, all wrangling, all subtlety. To make a show, and to appear learned and ingenious in natural knowledge, may flatter vanity; to know facts, to separate them from suppositions, to range and connect them, to make them plain to ordinary capacities, and, above all, to point out the useful applications, is, in my opinion, much more laudable, and shall be the object of my ambition." His course was copiously illustrated by preparations. He advised his students to take notes, not during a first, but during a second course. He recommended them to spend time in the practical rooms during the first year, but not to dissect till the second year of study.

I have not time to enter on the large subject of preparations, as they are called, and it is not necessary, for all the world knows and acknowledges what our hero and his brother John did in this way. They may almost be said to have commenced the era of great medical museums. William Hunter's own collection, including those of Sandys and of Hewson, is now in the University of Glasgow, having been taken there in 1807, after having been long used by Baillie, his nephew and successor. The collection formed by Wilson and Sir C. Bell, to fill the place of William Hunter's, removed from Windmill Street, is, with numerous additions, the museum of our College of Surgeons. The immense collection of John Hunter is, as is well known, the chief part of the great museum of the College of Surgeons of England. "I have collected," says William Hunter, "such an anatomical apparatus as was never brought together in any age or country."

The scientific progress in anatomy and physiology directly effected by William Hunter was very considerable. He contributed to the Transactions of the Royal Society, and to the "Medical Observations and Inquiries," at least twenty papers, of which the following is a list, for the compilation of which I am indebted to Dr Underhill:—

1743. On the Structure and Diseases of the Articulating Cartilages. (Philosph. Trans., vol. xlii. p. 514.)
1756. The History of an Aneurysm of the Aorta, with some Remarks on Aneurysms in general. (Medical Observations and Inquiries, by a Society of Physicians in London, vol. i. p. 323.)
1757. The History of an Emphysema. (Ibid., vol. ii. p. 17.)
1757. Remarks on the Cellular Membrane and some of its Diseases. (Ibid., vol. ii. p. 26.)
1761. An Account of a Diseased Tibia. (Ibid., vol. ii. p. 303.)
1761. Remarks on the Symphysis of the Ossa Pubis. (Ibid., vol. ii. p. 333.)
1761. Further Observations on a particular Species of Aneurysms. (Ibid., vol. ii. p. 390.)
1768. Observations on the Bones, commonly supposed to be Elephants' Bones, which have been found near the River Ohio, in America. (Philosph. Trans., vol. lviii. p. 54.)
1770. Remarks on some Bones found in the Rock of Gibraltar, in a Letter to Dr Maty, Sec. Roy. Soc. (Ibid., vol. lx. p. 414.)
1770. Introduction to a Paper on the Insensibility of Tendons, by Mr John Teckel, Surgeon. (Med. Observations, etc., vol. iv. p. 343.)
1770. Postscript to an Account of a Case of Varicose Aneurysm, by Mr Thomas Armiger. (Ibid., vol. iv. p. 385.)
1770. Appendix to a History of a Fatal Retroversio of the Uterus and Rupture of the Bladder in Pregnancy. (Ibid., vol. iv. p. 400.)
1771. An Account of the Nyl-Ghau, an Indian Animal not hitherto described. (Phil. Trans., vol. lxi. p. 170.)
1774 (or '5). Essay on the Origin of the Venereal Disease. (Read at Roy. Soc. Never published. See Life by Simmons.)
1776. Summary Remarks on the Retroverted Uterus. (Medical Observations, etc., vol. v. p. 388.)
1777. A Short Account of Dr Maty's Illness, and the Appearances in the Dead Body, which was examined on 3d July 1776, the day after his decease, by Dr Hunter and Mr Henry Watson. (Phil. Trans., vol. lxvii. p. 608.)
1778. Reflections on the Section of the Symphysis Pubis. (Read before Roy. Coll. of Phys., and then published. See Life by Simmons.)
1780. Letter to Royal Society in Answer to John Hunter, claiming the Discovery of the Anatomy of the Placenta. (Unpublished. See Life by Simmons.)
1783. On the Uncertainty of the Signs of Murder in the Case of Bastard Children. (Posthumous. Med. Observ., vol. vi. p. 266.)
1783. Three Cases of Malconformation of the Heart. (Posthumous. Med. Observ., vol. vi. p. 291.)
1783. The Successful Cure of a Severe Disorder of the Stomach by Milk taken in Small Quantities at once. (Med. Observ., vol. vi. p. 310.)

Besides these papers, he published a volume, and subsequent supplement, of "Medical Commentaries," and projected a series of such, but never produced a second. His discoveries regarding the lymphatic system, its distribution and uses, he believed to be the greatest, "both in physiology and in pathology, that anatomy has suggested, since the discovery of the circulation." This great work, he says, he completed with the assistance of his brother

John, of Hewson, and of Cruikshank. He counted also among his great works the description of the varicose aneurism, and of the retroverted uterus, with methods of treatment. His successor in Windmill Street, and I believe also his relative through Denman, the late Sir Benjamin C. Brodie, signalizes his early work on the synovial membrane as anticipating all that Bichat wrote on the subject sixty years afterwards; and he mentions his paper on the uncertainty of the signs of murder in the case of bastard children as a remarkable display of intellectual penetration, great good sense, and power of argument. After his death, his nephew Baillie brought out a volume, consisting of two lectures introductory to his last course of anatomical lectures at his theatre in Windmill Street, as they were left corrected for the press by himself; to which are added some papers relating to his plan for establishing a museum in London. Baillie also brought out a little imperfect volume on the anatomy of the gravid uterus, which had been probably intended as a descriptive text for the plates.

The greatest of his works is his well-known Plates of the Gravid Uterus; so well known, indeed, that I shall say little about it. Upon this he lavished his mind and his purse, and it embodies his great discoveries as to what he termed the decidua, and as to the structure of the placenta, and regarding the retroverted uterus. "They make," says he in a letter to Cullen, "thirty-four large plates. It will be a very considerable work for expense and show. Perhaps it will be the most considerable in that way that will ever be published, so few men can have the same opportunities or better than I have had. After that, if I am in health, I shall publish many other things. I only want time, and in less than two years I hope to be doing *only a little* business." In another place he expresses his obligation to Mr Strange, "for having by his hand secured a sort of immortality to two of the plates." I could give much more evidence, were it necessary, to show how he availed himself of the highest talents to be reached in Europe, in order to secure perfection in this book, and his attention ranged over all the departments, from dissecting to drawing, engraving, printing, and paper. The excellence of Hunter's Latinity has often been remarked. The work is indeed, as yet, an unrivalled performance.[1]

I have already spoken of William Hunter's museum and preparations as among his anatomical works, and I might have enlarged upon other improvements in anatomical apparatus and method which he introduced or modified, especially the art of injecting. But I am myself too little acquainted with these practical details to do them any justice. I cannot, however, pass on without asking attention to the great progress of anatomy and physiology, which we owe partly or indirectly to William Hunter, through the instruction of assistants, the direction of their labours, and the communication to them of his own enlightened enthusiasm. William Hunter takes merit for himself in this department of activity, and, when the matter is inquired into, there can be no doubt of the justice of the claim. Among his followers or scientific descendants in the Windmill Street School, which he founded, were Matthew Baillie, James Wilson, Sir Benjamin Brodie, Sir Charles Bell, and Herbert Mayo. It is not to these we refer, but to those whom he trained and guided under his eye, John Hunter, William Hewson, William Cruikshank, and John Sheldon.

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[1] On this subject there ought, had time permitted, to have been much more said. The author may refer the reader to some criticism on W. Hunter's work, in his book entitled "Researches in Obstetrics," p. 222. But that criticism does not fully supply the deficiency in this part of the discourse.

John Hunter, the youngest of his father's family, an ill-educated, rude lad, he brought to London, and set to work as an anatomical assistant, especially in making preparations. With what ultimate result all the world knows. "On the same principle," says Sir Benjamin Brodie, "on which Ulysses is made to lay claim to part of the glory belonging to the achievements of Achilles, William Hunter deserves to be honoured on account of those of his brother John—

'Ergo opera illius mea sunt.'

He brought the uncouth Scotch lad to London, who afterwards became one of the greatest philosophers of this country, but who might otherwise have remained to be a farmer in his native county of Lanark. It was as his pupil that John Hunter pursued his earliest studies in his profession; and it was under the influence of his example that he learned to cultivate as a science what was before little more than a practical art and trade. I am inclined to believe," he adds, "that I should not at all exaggerate what we owe to John Hunter, if I were to assert that, with the exception of Sir Isaac Newton, there has been no individual, in these latter times, who has done so much as he has done towards altering and elevating the character of the peculiar sciences to which he devoted his attention." . . . "By teaching us when we are not to interfere with the ordinary course of events, he has contributed more towards the advancement of the healing art than all the inventors of remedies who had gone before him."

These last words of Brodie's evince their author's penetration and wisdom, and express very aptly the tenor of the praise which is given to William by obstetricians as it is here given to John by this distinguished surgeon.

I shall conclude this part of my eulogium on William Hunter by quoting his own words regarding his three great and famous assistants.

"My brother, Mr John Hunter, whom I bred to practical anatomy, and who worked for me, and attended my dissecting-room, and read some lectures for me many years."

"Mr Hewson, whom I first bred to anatomy, and then took into my house to work for me, and under my direction, in practical anatomy, to attend my dissecting-room, and read some lectures as my partner, which he did for a number of years. Mr Hewson, by a continued course of observations and experiments made in this house (in Windmill Street), discovered and fully demonstrated the lymphatics and lacteals, both in birds and fishes," etc.

"Mr Cruikshank, whom I likewise bred to anatomy, and took into my house upon the same plan, with the opportunities which he has had in this place, and by being particularly attentive to the lymphatic system, at my desire, has traced the ramifications of that system in almost every part of the body; and from his dissections, figures have been made, which, with what I had before, will enable us to publish (we hope in a little time) a full account of the whole system, illustrated by accurate engravings."

To William Hunter's great success in practice, and especially as an accoucheur, I shall only briefly allude; for no amount of lucrative practice, no consideration of the exalted station of patients, can establish for their fortunate and rich possessors one tittle of claim to posthumous glory, or entitle them to be the subject of a Harveian address. In the words of Holy Writ, "They have their reward." Their pockets are full, and they have during life crowds of gaping admirers. Contemporary fame and fortune come to many a worthy and to many an unworthy man. William Hunter deserves admiration, but not as a successful practitioner in the vulgar sense. One of his highest distinctions is the use he made of success in practice. The kind of prosperity in practice which he coveted is well given in a passage of one of his introductory lectures, when directing the attention of students to the close of their lives; he says, "I am certain the most diligent, the most conscientious, and the most humane among you all, will most ardently wish that you could have done still more service to the cause of your poor distressed fellow-creatures." We know that he regarded the advance of science as the best means of promoting the healing art, and his life was devoted to this, as its whole history, and the posthumous destination of his great fortune, abundantly demonstrate. He repeatedly speaks of curtailing his practice to allow him to spend more time at higher work. I have quoted a letter from him to Cullen, where he says he trusts soon to have "only a little business." "For the future," he says in another place, "money can be of no use to me, but for acquiring and communicating science; which shall be my object as far and as long as I can pursue it." I have repeatedly heard a great and recently deceased teacher of clinical surgery in this School use words almost the same as the following of William Hunter:—"A man may do infinitely more good to the public, by teaching his art, than by practising it. The good effects of the latter must centre in the advantage of the few individuals that may be under his care as patients; but the influence of a teacher extends itself to the whole nation, and descends to posterity."

These just views as to the paramount importance of science and of teaching deserve more attention than is given them, and require to be inculcated on the profession of our day quite as much as in William Hunter's time. Were we to judge, by their conduct, the teachers in some of our greatest schools, we should think practice and money-making the noblest objects of ambition, for we find it the almost invariable custom to give up the pursuit of science and the occupation of teaching as soon as the scent of fees becomes strong. A little devotion to science to get a good position as teacher. A good position as teacher to have introduction to practice. When practice begins to be lucrative, then adieu to science and to teaching also. Such was not the ambitious climax of William Hunter. And the last object of his endeavours was to secure for medicine proper teachers, and to prevent this occupation being entered upon as a stepping-stone to practice. "Without some public and permanent foundation, he foresees that anatomy, and everything that depends upon it, must sink again to its former state. It will be taught," he says, "only by young men, as an introduction to business; the name of lecturer, in newspapers, and in private conversation, never failing to give a man some degree of credit. But such young teachers will generally be very indifferently qualified when they begin; and when they have acquired some ability from experience, that is, when they are just become fit for teaching, they will generally leave it off. They will always find their labour better rewarded (in the vulgar sense of reward) by following the practice of physic or surgery, than by reading lectures."

I conclude with a few words on the comparison frequently drawn between William Hunter and his younger brother John. The generally entertained opinion is well expressed by Mr Babington as follows:—"The utility of the comprehensive system adopted by John Hunter cannot be better shown than by comparing him with his brother Dr W. Hunter. Dr W. Hunter greatly surpassed his brother John in education, in method, in clearness of conception, in powers of reasoning, in facility and elegance of expression. He was also eminent for industry, and enthusiastic in the pursuit of science. Yet he has left behind him scarcely anything to perpetuate his memory, except the work on the Gravid Uterus, which, though undoubtedly of great merit, has had no very extensive influence on the progress of knowledge, and cannot in any manner be compared with what has been effected by his brother. The one great distinction was this, that, while Dr W. Hunter confined his inquiries for the most part to human anatomy and human pathology, those of John Hunter were carried through every part of the animal creation." In this statement of contrast every element that detracts from William's merits appears to me to be much overstated, while there is the entire omission of the best and greatest features of his character. I shall therefore cite a more favourable and more just judgment, that of the late Sir Benjamin C. Brodie :—" In making a comparison," says Sir Benjamin, " between the two Hunters, it seems difficult to avoid giving the pre-eminence to the younger brother; but that pre-eminence did not belong to him in everything. I have described what William Hunter was as a lecturer, but John Hunter did not excel in this capacity. It was not always easy to comprehend his meaning. William Hunter was a man of learning, and an accomplished writer: John Hunter had no learning, in the common acceptation of the term; he composed with difficulty, and no small effort of attention is sometimes required to enable us to comprehend his ill-arranged and involved sentences."

Perfect fairness is scarcely to be expected in these quoted contrasts, for both judgments are given in solemn panegyrics of John. To ascribe to William a superiority merely in matters of taste is no doubt a liberal meed of praise, but I believe I have in this address given sufficient evidence that the comparative merits of William are not fairly stated by claiming for John unrivalled superiority except in this respect. Ever since I began to teach medicine I have taken care to point out how the great and well-earned fame of John has cast an injurious shadow on the reputation of his elder brother. Yet I do not go the length of agreeing with one of the greatest living physiologists, who, in a recent conversation on this subject, avowed his conviction that, of the two brothers, William was the greater. In conclusion, as I have given the views of two men as expressed parenthetically in the praises of a rival, I shall give the unbiassed estimate of a well-qualified judge, Dr Thomson, the biographer of Cullen.

" Few are the individuals, indeed," says he, " of whom it can justly be said, that their labour, their example, and their actual service have conferred so great benefits upon science, and tended in a degree so remarkable to improve and facilitate the means of communicating the correct knowledge of the facts of anatomy and the principles of physiology and pathology, as William Hunter and John Hunter. It may, without exaggeration, be said that both England and Scotland owe to William Hunter, and his brother John Hunter, a debt which has never yet been repaid, and which probably is too great to be repaid. Both were, in the most true sense of that term, benefactors to the human race. Both laboured not only without public encouragement, but in the face of the greatest difficulties and the most chilling and discouraging inattention, in teaching anatomy and physiology, and the just principles of pathology, upon a foundation solid and durable. And both devoted, in the most disinterested manner, the pecuniary means which the exercise of their profession supplied, not in procuring expensive luxuries, not in acquiring large estates and erecting magnificent mansions, but in providing for subsequent generations the means of anatomical instruction upon the same solid foundations on which they had already taught, and the superior advantages of which they had proved by their own example."

It is for us, Gentlemen, to admire them both, to cherish their memories, and emulate their good deeds.

With these words I draw abruptly to a close my remarks on the life of William Hunter. Much more might be said of this great man on the anniversary of the birth of Harvey, which we are to-day met to celebrate. Yet I hope that, at least, the spirit in which I have addressed you will meet your approval, however imperfectly I may have executed the task I proposed to myself in composing this essay. The spirit which has moved me, and which I have tried to communicate to you, by holding up the great example of William Hunter, is the spirit which guided William Harvey, and which he aimed at perpetuating after his death—the spirit of science and of mutual love. Harvey left funds for the same purpose as brings us here to-day. "To maintain friendship," says he, "there shall be once in every year a general feast, . . . and on the day of such feast shall be an oration." This oration, he says, is to commemorate benefactors; and then come, in his will, the following memorable words, which I most solemnly and sympathetically repeat in your ears. The oration is to be, says Harvey, "with an exhortation to others to imitate, and an exhortation to the members to study and search out the secrets of nature by way of experiment, and for the honour of the profession, to continue mutually in love."

ARTICLE II.—*Diphtheria: Typical Cases, in illustration of its Diagnosis, Prognosis, and Treatment; together with Remarks on the Signification of Albuminuria in that Disease.* By Sir JOHN ROSE CORMACK, K.B., F.R.S.E., M.D. Edin., M.D. Paris, Fellow of the Royal College of Physicians of London, Chevalier of the Legion of Honour, Physician to the Hertford British Hospital of Paris.

(*Continued from page* 995.)

TREATMENT.

THE treatment of diphtheria requires to be considerably varied in its details, according to the nature of each case, the constitutional peculiarities of the patient, and the type of the epidemic. There are, however, certain general principles of treatment which must always be acted upon, and the infringement of which may lead to disastrous consequences.

Even a limited experience will teach an observant practitioner not to expect curative results in diphtheria from particular medicines or vaunted formularies of treatment, but to strive to support life by the measures best suited to each case, rationally using medicines as exigencies and opportunities arise, and not in a routine fashion. The first Begbie of Edinburgh, and, I may say, the best physicians who have given their views on this subject to the profession, express themselves to that effect. Begbie, whose skill as a therapeutist stood very high, concludes the summary of his able and instructive essay on "Diphtheria and its Sequels" in the following sentence:—"Lastly, as we have no specific remedy for diphtheria, the disease and its sequels must be treated on the general principles which regulate our practice in fever, in inflammation, and in nervous disorders of asthenic character."[1]

The treatment of diphtheria may be conveniently discussed under the three heads of *general*, *local*, and that which pertains to the *paralytic affections of convalescence.*

The *general treatment* has to be considered in respect to *atmosphere, food*, and *medicines.*

The temperature of the room ought to vary as little as possible, a temperature of about 17° Cent. (63° F.) being maintained. The patient ought to be screened from currents of air, care being taken that free ventilation is not interfered with, and that the air is moistened by a regulated escape of steam from a suitably-contrived kettle. The arrangement adopted in the case of E. G. answered very well. Nothing can be better for the purpose required than Dr Pretty's kettle, which is thus described by Sir William Jenner:—"This is a tin kettle with a small aperture at the top, closed by a screw instead of a common lid. From the front of the kettle project two spouts of about three feet in length. One spout springs from the upper part of the kettle, and passes forward in a straight line; the other spout springs from near the bottom of the kettle, and passes obliquely upwards. The lower spout ends in a spoon-like projection, just under the slightly curved-down open mouth of the upper spout. The steam passes out of the upper spout, and the condensed vapour drops into the little spoon, and is returned by the lower spout to the bottom of the kettle."[2] A thermometer and a steaming-kettle are indispensable in the chamber of the diphtheritic patient. The maintenance of good ventilation, combined with a moist, warm, and equal temperature, is a paramount necessity when tracheotomy has been performed; and in all cases, and in all stages of the disease, in which there exists diphtheritic sore-throat, it is important, as a means of moderating the paroxysms of glotto-laryngeal spasm, that the patient inhale air which is soft, warm, and equable in temperature. Even in the rare cases in which the throat affection is absent, it is the duty of the physician to take the measures best calculated to secure in the sick-room such an atmosphere as has been described; for in such cases the disease may at any moment manifest itself in the air-passages.

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[1] Contributions to Practical Medicine, by James Begbie, M.D. Edinburgh, 1862. See p. 266.

[2] Sir William Jenner.

The support of life by stimulants and aliments—the feeding of the patient—is universally stated to be an essential element in the treatment of a case of diphtheria. Neither alimentation nor tracheotomy were *curative* agents in the case of E. G.; nor in any case of diphtheria can they be so regarded. Nevertheless, they were the principal means by which E. G. was saved from death; and by them, indeed, is recovery chiefly rendered possible in all such cases. Success in alimentation, and success in tracheotomy, are only means by which we gain time, by which we support life for a period, we hope, of sufficient duration to enable the disease to run its natural course, guided and aided by us whenever therapeutic opportunities arise.

It is necessary to insist emphatically on the fact, that, in the treatment of diphtheria, there is nothing approaching alimentation in importance. Unfortunately, however, this knowledge is too often of little importance to physicians and patients in bad cases, for in such there is almost no power of assimilation, and there is likewise extreme difficulty in inducing the patient to take food, or having taken it, for him to retain it. Diphtheria-stricken patients generally loathe food, and children often struggle violently against attempts to feed them. When food is swallowed, it is often rejected immediately. The difficulties in the way of feeding are always great, and sometimes they are insuperable; but, still, they must be resolutely faced. The alimentation of diphtheritic patients requires great skill, tact, and, I might almost say, inventive power on the part of the medical attendant, assisted by the co-operation of a well-disciplined, conscientious, and obedient nurse. Each case has dietetic difficulties which are its own, and must be met from hour to hour as they arise.

While, therefore, it would be tedious to go into details, a short statement of the practical principles which require to be carried out may be briefly stated. Pounded raw beef in very small quantities, moistened with the juice of under-done roast-beef, is generally the best basis of alimentation. It will seldom be expedient to administer more of this preparation than one teaspoonful at a time, and not nearly so much if there be nausea. With the raw beef and other aliments, a little *pepsina porci* ought to be given from time to time. I have seen the difficulties of alimentation much diminished by the judicious addition of pepsine to the food. Together with the raw beef and other aliments, we must give stimulants liberally: the exact quantity must be determined by the exigencies of each case, and will be subject to frequent variations. As a general rule, however, it is well to remember that brandy is well borne in diphtheria by patients of all ages. Its effects require to be carefully observed in young subjects; but it may be accepted as a fact, that children bear brandy, sherry, and all spirituous stimulants exceedingly well. Proofs of the accuracy of this statement constantly present themselves in practice, both in respect to diphtheria and other diseases.

"When," says Sir William Jenner, "the disease begins with marked feebleness of pulse, dusky redness of throat, and extreme sense of general weakness, wine in full quantities is required at an early period. From six to eight ounces of sherry or port for an adult, and as good a diet as the patient can take, must be given from the first. In the course of the disease much larger quantities of wine, or a proportionate quantity of brandy, may have to be given. Of course, the quantity of stimulant must be regulated by the age and habits of the patient, as well as by the character and the stage of the disease; but remember that, as a rule, young children bear and take with advantage, in diseases of depression, much larger quantities of stimulants than you would probably suppose. A child of three years of age, now under treatment for diphtheria at the Children's Hospital, is taking, with apparent advantage, from one to two drachms of brandy every hour, *i.e.*, from three to five ounces of brandy in twenty-four hours."

When we have nausea and vomiting to contend with, we must chiefly trust to brandy and pounded raw beef (duly pepsinated) as the dietetic articles most fitted for keeping up life. When the stomach will bear more bulky food, it is always useful to give a variety of suitable aliments, among which may be mentioned milk, egg-flip, and panada. As soon as it can be borne, cod-liver oil ought to be given. It has a wonderful power in preventing and restoring the waste of tissues.

There is very little if any scope for the administration of medicines when a bad case of diphtheria is at its worst. Till the fury of the disease has spent itself, it is wise to give as little medicine as possible, and never to give any at all unless the indication be clear and positive. When there is nausea and vomiting we may harmlessly and hopefully give oxalate of cerium or creasote, but we must avoid, on account of its depressing influence on the heart, the other great remedy for irritability of the stomach—hydrocyanic acid. As soon as the patient can digest it, iron in some form ought to be given in very small doses. It may be very usefully combined with a syrup of the phosphate of lime. Ferruginous medicines are urgently demanded from the very dawn of convalescence by the anæmic aspect of the patients, while cod-liver oil and phosphate of lime are equally called for by their emaciated appearance. Building-up treatment, alimentary and medicinal, is most useful in preventing or moderating the paralytic affections incident to advanced convalescence.

There is no specific medicine for diphtheria—there is no way of *curing* that disease; but there are many medicines and many measures of signal benefit to diphtheria-stricken patients, by the skilful use of which they are often enabled to recover.

With the use of general means, it is sometimes proper in laryngo-tracheal manifestations of diphtheria to combine local treatment to dislodge or dissolve the false membrane. The treatment by emetics adopted for the former purpose is local in its intention, but general in its action on the patient.

Emetics in diphtheria are seldom of much use; but still there are many cases in which it is right to try to effect dislodgement of the false membrane. With that object, an emetic was administered to Miss P. on Sunday, 2d January (p. 805); and for the same purpose emetics were given to E. G.

The emetics which ought to be selected are those which do not depress, and which act quickly. Perhaps sulphate of zinc is the most, and tartar emetic the least, suitable. The latter is not only unsuitable, but is pretty certain to prove dangerous by its depressing action. Speaking of tartar emetic as an emetic in diphtheria, Trousseau says:—"The selection of the particular emetic to be employed is not a matter of indifference. Tartar emetic, so lauded by some, seems to me to be the most dangerous of all emetics." . . . "It causes extreme prostration and accelerates death."[1] Trousseau's teaching, unfortunately, is not universally followed in this matter, as I have had several occasions to observe. The following instance is confirmatory of Trousseau's statement. All the circumstances being remarkable are accurately remembered by me.

On a summer morning in 1875, I accompanied my friend Dr Borthwick of Dumfries on a visit to one of the great hospitals of Paris. During a long drive to the hospital, the chief subject of our conversation was the pathology and treatment of diphtheria in relation to emetics and tracheotomy in the laryngo-tracheal manifestation of the disease. We knew nothing of the cases we were to see. On our arrival we entered a medical ward where a physician was examining the first case of diphtheria, we were told, which had been received into the hospital during the current year. The sick man, aged about forty, was sitting half-dressed on his bed, a circumstance explained by his having just returned from the privy situated outside the ward. We ascertained that he had been about forty hours in the hospital, but did not learn the previous duration of his illness. Since admission, he had had low diet and no stimulants. He spoke in a husky whisper. He had had no stridulous breathing. From his replies to questions, we ascertained that he chiefly complained of dyspnœa, diarrhœa, loathing of food, and debility. The visible part of the interior of the throat was covered with false membrane; and the physician announced, after applying the stethoscope, that he heard semi-detached false membrane flapping in the trachea. The treatment prescribed consisted in a continuance of the antiphlogistic regimen, and the exhibition at short intervals of tartar emetic in doses of ten centigrammes. I do not know whether it was the prescriber's object to obtain the dynamic action of the drug, or whether its emetic effects were looked to as a means of dislodging the false membrane from the air-passages. Before we left the ward, the patient went to and from the privy with tottering steps. The exertion induced extreme vital depression, unaccompanied by stridulous breathing or increase of dyspnœa. Dr Borthwick and I agreed that the only chance—and that a very small chance—of recovery which this man possessed consisted in his being kept in the horizontal position, and liberally dosed with brandy, an emetic of sulphate of zinc being delayed till a rally should occur, and tracheotomy being resorted to only if it should be demanded by threatening asphyxia. We were equally agreed that under the combined depressing influences of diphtheria and antimony, it was not likely that the patient could survive more than a few hours. Our evil prognosis was correct, for the patient died in a state of collapse six hours after our visit.

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[1] Trousseau, op. cit., vol. ii. p. 578.

The tartar-emetic treatment of diphtheria has been generally regarded as one of the wildest heresies in the practice of medicine, though some able men of large experience think and teach otherwise.

In 1859, during the prevalence of a severe epidemic of diphtheria at Paris, three cases were reported as having been treated successfully by Bouchut at the Sainte-Eugénie Hospital by large doses of tartar emetic. The three patients took the medicine according to the following formula:—Tartar emetic, 75 centigrammes; syrup of poppies, 15 grammes; and gum-water, 100 grammes; mix; half a a teaspoonful to be taken every hour. The quantity intended to be taken in a day was from 50 centigrammes to a gramme—that is, from $7\frac{1}{4}$ grains troy to $14\frac{1}{2}$ grains troy of the tartar emetic. There were two objects in view—the excitation of vomiting as a means of getting rid of the false membrane, and the mastery of the disease by the successive dynamic shocks. It is stated that the nurse, observing one of the three patients in a suffocative paroxysm from the presence of laryngeal false membrane, gave a double dose of the mixture with the addition of some tepid water. Forthwith, the child in a violent vomitive effort ejected a tubular membrane, two inches in length.[1] The incident is interesting, but it does not tend to justify the administration of tartar emetic in laryngo-tracheal diphtheria. Would it not have been equally efficacious, and much safer, to have administered an emetic dose of sulphate of zinc, followed by some brandy to sustain the feeble heart during the vomitive crisis?

Tracheotomy, through the opposition of the family, was too long delayed in the case of E. G. A similar difficulty often occurs in private practice. Each case has to be decided for its own merits; and the physician in charge must be in constant readiness with his instruments and appliances to perform tracheotomy at very short notice. In the majority of cases the actual crisis is so sudden as to leave no time to divide responsibility with a colleague. The patient must not therefore (if the attendant can help it) be put in jeopardy by waiting for a formal consultation, or till a surgeon can be found to admit oxygen to the craving lungs. On the other hand, if time permit, there is no emergency in medical practice in which it is more for the advantage of patient and practitioner that there should be a collation of opinions and a division of responsibility.

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[1] Bouchut, L'Union Médicale, for 5th April 1859.

In the diphtheritic semi-asphyxiated child, tracheotomy is an operation requiring great care and a good light. There is no surgical difficulty, but the operator, if unaccustomed to use the knife, must be cautious. Nay, even an expert requires to proceed slowly, for children with turgid necks have been lost from hurried tracheotomy, performed with imperfect light, by good operators. The difficulty and danger of tracheotomy in diphtheritic children arise from the turgidity of the veins of the neck, caused by the state of semi-asphyxia. The sudden gush of venous blood which occurred in the case of E. G. illustrates this remark and confirms its correctness.

The patient ought to be placed on his back on a table, with a narrow solid cushion so adjusted under the neck as to project and stretch the trachea. A quart bottle wrapped up in wadding, or in anything at hand, answers admirably. This being arranged, the operator, with the least possible delay—for the patient's position is a very trying one—makes an incision through the skin, in the mesial line, from the cricoid cartilage nearly to the sternum. The tissues ought then to be divided layer by layer, the gorged veins being carefully avoided, and the muscles and vessels being held to each side by the fingers of the left hand of the operator, or by two blunt hooks held by an assistant. When the trachea has been laid bare, a small incision is made in it, close to the cricoid cartilage, with a sharp-pointed bistoury, after which a probe-pointed bistoury is employed to complete the necessary opening. By means of the tracheotomy-dilator, or if that be not at hand, by means of a common dressing forceps, the opening is dilated, and the operation completed by introducing a double canula, and then fastening it behind by tapes.[1] As in the case of E. G., it may be necessary to draw out detached portions of false membrane before the canula can be introduced. In such cases, it is well to keep the opening dilated till the false membrane and mucosity have been got rid of by coughing or otherwise. The inner canula in some cases requires to be frequently removed and cleansed from obstruction. For such emergencies and for such occurrences as the grave accident which befell E. G., on the third night after the operation, a reliable attendant must be ready to intervene at a moment's notice.

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[1] It is not safe to operate in this class of cases without skilled assistance, unless prepared single-handed to tie vessels accidentally wounded. This is easily managed by being provided with two or three of the short light artery forceps made for service in such special emergencies. During the late war, relying on those instruments, I repeatedly performed considerable operations without skilled assistance, by instantly securing the vessels as they were divided, and allowing them to remain in the grasp of the forceps till it was convenient to apply ligatures.

Another method of performing tracheotomy in diphtheria has recently been made by Saint Germain, of the Hôpital des Enfants Malades of Paris. The object in view is to avoid hæmorrhage from cutting the engorged veins. A red-hot probe-pointed bistoury is the instrument employed. It is used in the first instance to burn through the skin, intervening tissues, and crico-thyroid membrane; and then, by using the cutting edge, to divide the cricoid cartilage, and a few rings of the trachea. With the aid of Lalonde's dilator, the canula is then introduced.

Tracheotomy, like venesection, and the use of the stomach-pump, is a mechanical service, which every one who assumes the responsibilities of medical practice ought to be able to render to his patient at once, whenever the emergency arises. The question is not whether tracheotomy belongs to medicine or to surgery—that is of secondary importance—but whether every man ought not to save life when he can do so, by the use of his hand. It is expedient that some should specially cultivate medicine, and others specially cultivate surgery; but it is a great scandal when a physician in certain emergencies refuses to use the surgical knife, and when a surgeon in certain emergencies refuses to write a medical prescription.

Local Applications intended to destroy, detach, or dissolve the false membrane in laryngo-tracheal diphtheria are in favour with many. Fortunately they are not so much relied on now as they were by Trousseau and those who wrote by his inspiration. This change of opinion is, as yet, more apparent in the conversation and current practice of French physicians than in their published works. It is now generally admitted that Trousseau attached an undue, and even a dangerous, importance to destroying by caustics the false membrane as soon as it appeared on the pharynx, and on any part of the visible mucous membrane of the throat. His statement that the destruction of the false membrane not only prevented the spread of the local mischief, but even arrested the career of the general disease itself, is now denied by most French clinicians of repute. This change of opinion is fortunately likely to be permanent, for it has been clearly shown, and is now generally believed, that caustics, strong acidulated washes, and active chemical solvents, act mischievously by irritating the mucous membrane, and so exciting increased exudation of cacoplastic lymph.

Gargles, washes, and various other applications, if not of an irritating character, may be used with impunity, and sometimes with benefit. Some of them tend to promote separation of the false membrane without producing any rawness or hurtful irritation of the subjacent mucous membrane. The advantage derived from them is, we must remember, frequently temporary, and more apparent than real. So long as the disease is in the exudation stage, layer after layer of false membrane will continue to be deposited on the surface of the mucous membrane ; and the rapidity with which this reproduction proceeds may more than counterbalance the benefit derived from the separation of the upper strata. It follows, therefore, that the only topical applications to be used are those which do not irritate.

Among the safe and more useful topical applications are glycerine and borax (of the *Br. Ph.*), lime-water, a very diluted solution of hydrochloric acid in distilled water, and a solution of one drachm of neutral sulphate of soda in eight ounces of water.

Moist warmth applied externally to the throat generally gives much comfort, and is in no way injurious. It greatly mitigates the pain arising from tumefaction of the cervical glands.

(To be continued.)

ARTICLE III.—*Case in which a Reducible Oblique and Direct Inguinal and Femoral Hernia existed on the same side, and were successfully treated by Operation.* By THOMAS ANNANDALE, F.R.S.E., Surgeon to the Royal Infirmary, and Lecturer on Clinical Surgery.

(*Read before the Medico-Chirurgical Society of Edinburgh*, 3d *May*).

J. S., æt. 46, was sent to me on the 3d of January last, by Dr Maclagan of Berwick, for advice in regard to a complicated hernia. Two years before an inguinal hernia appeared on the right side, and gradually increased in size and passed down into the scrotum. A few months after a second hernia showed itself in the femoral region, and this hernia has also steadily become larger. The patient was not able to wear any truss which controlled the protrusions, and consequently the herniæ always came down when he stood, walked, or coughed. In addition, he suffered much from dragging pain in the region of the umbilicus, and his condition completely prevented him from following his employment as a labourer.

Having admitted the patient into my wards in the Royal Infirmary, an examination determined the presence of a large reducible inguinal hernia on the right side. The hernia passed down into the scrotum, and was of the size of two closed fists.

On the same side there was a reducible femoral hernia, the size of a large orange. Both herniæ were easily reduced, but the slightest cough or movement brought them down again.

Knowing the impossibility of successfully applying a truss in such a complicated case I suggested to the patient that an operation might afford him relief and assist the application of some mechanical support. Having given his consent, I performed the following operation on the 14th of January:—

An incision about two and a half inches in length was made half an inch above and parallel to the inner half of Poupart's ligament. This incision was carried through the skin, fascia, and muscles so as to expose the necks of both inguinal and femoral herniæ close to their junction with the general peritoneum, and outside it. The epigastric artery was ligatured and cut across, as it was included in the incision.

The object of my operation being to ligature the necks of the sacs with catgut as close to the general peritoneum as possible, I first drew out the inguinal sac, which was non-adherent. When this sac, its contents having been reduced, was drawn out of the wound, it measured about six inches in length, and there was closely connected with it a second and partially obliterated sac. This second sac was carefully separated from the first, and when examined appeared to be the sac of what had been a direct inguinal hernia. Having made a small incision into both of these sacs, and ascertained that their contents were reduced, I applied a catgut ligature around their necks as high up as possible.

I now prepared to act in the same way upon the neck of the femoral sac, but found that it had slipped up into the abdominal cavity. I could pass my finger readily by the wound into the femoral canal, down which the hernia came; and, while hesitating how best to secure the femoral sac, it occurred to me that the ligatured inguinal sacs might be drawn down into this canal and secured there, so as to plug it up and prevent the descent of the femoral hernia.

Accordingly, having passed my finger down through the wound into the femoral canal, I introduced through the inner aspect of the thigh, at a point a little internal and below the saphenous opening, a curved needle armed with carbolized silk, and pushed it through the tissues until its point reached my finger in the femoral canal, when it was brought out through the wound, together with one end of the silk, the other end being left protruding from the thigh. The needle with the silk attached was now run through and through the lower ends of the inguinal sacs, and, being carefully guarded by the finger, was re-introduced through the wound into the femoral canal, and along with the silk was brought out again on the thigh close to its original point of entrance. By pulling upon the two ends of the silk the inguinal sacs were drawn down into the femoral canal, and secured there by tying the ends of the silk together over a piece of indiarubber cord.

When the wound was stitched up, the patient was made to cough and move, but no protrusion of either hernia took place. The whole operation was performed with antiseptic precautions. No bad symptoms interfered with the good progress of the patient, and the stitches were removed on the tenth day. Two weeks after the operation the silk thread was cut, and came away quite easily, after a little traction had been made on it.

On the 12th of February, a truss, having two pads, one to press over the inguinal region, the other over the femoral region, was applied.

On the 14th of February, the patient rose from bed wearing the truss, and although I removed the truss when he was in the upright position, and tested him in various ways, no protrusion of the hernial tumours took place. From this date he continued to walk about the ward freely with the truss; and on the 26th of February he returned home, quite relieved from his miserable condition, and able to walk and move perfectly.

On the 3d of May I had an opportunity of examining the patient, who had been following his employment since he returned home with perfect success. The inguinal protrusion no longer existed, and coughing and other movements caused no hernial descent in this region. There was a slight protrusion in the femoral region, which was, however, easily controlled by the pad of the truss.

Remarks.—I record a note of this case,—*First*, Because it is an example of a rare form of multiple herniæ affecting the same side. It is not very uncommon to meet with cases in which a hernia protrudes at more than one of the usual hernial sites, but the condition in which both a direct and inguinal hernia affect the same side is a rare one.

Lawrence[1] writes—"Internal and external hernia may coexist on the same side; and these two may occur in conjunction with femoral hernia of the same side." He further states that—"Cases are recorded in which a common inguinal and a congenital hernia have existed together on the same side. The complication is probably very rare." The same author refers to a case examined by Sir A. Cooper, in which there were three protrusions in each inguinal region, and all of them internal to the epigastric artery.

The best, and I believe the only, detailed description of double inguinal hernia is that recorded and figured by Mr John Chiene.[2] The preparation taken from this case is now in the Edinburgh University Museum, and shows very distinctly a well-developed direct and oblique inguinal sac on the same side. In Mr Chiene's case, the complicated nature of the hernia was not discovered until the parts had been dissected; and in my case, no peculiarity was observed in the inguinal hernia until the neck of the sacs had been exposed. The explanation of this probably is, that the one sac had become in great part obliterated, and therefore did not admit of the descent into it of any of the abdominal contents.

Although this double form of inguinal hernia is thought to be very rare, I think it not unlikely that it may really be more common, but have escaped notice during life, owing either to the smallness of one sac, the obliteration of one sac, or to one sac being pushed aside and obscured by the other.

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[1] A Treatise on Hernia, page 237.

[2] Edinburgh Medical Journal for March 1869.

The fact that such a double hernia may exist, is an important one to the practical surgeon, as it might be met with as a cause of an obscure intestinal obstruction, the presence of one unstrangulated hernia masking the existence of a second and strangulated one. Lawrence[1] refers to a case in which a Mr Wilmer operated on a congenital hernia, in which a common inguinal hernia also existed on the same side. The congenital one was operated upon, but the symptoms of strangulation continued, and the patient died. The second sac, containing gangrenous intestine, was found at the post-mortem examination.

Second, I report this case because it illustrates the success of an operation performed for the relief of a complicated case of reducible hernia, and because I am not aware that any previous attempt has been made by surgeons to relieve in this way, and at the same time, both an inguinal and a femoral hernia.

Although I am no great advocate for the so-called "radical" operations for the relief of reducible hernia, I believe that there are certain cases of reducible hernia, not easily controlled by the application of a truss, which may be much benefited by an operation, and placed in a condition which will allow a truss to be satisfactorily employed. The case reported certainly proves the value of such an operation.

A knowledge of the pathological fact that contraction or obliteration of the neck of a hernial sac is not uncommon, and the consideration that this condition is undoubtedly a cause of the spontaneous cure of reducible hernia, lead me to adopt ligature of the neck of the sac in my operation. As is well known, ligature or compression of the neck of a hernial sac is no new operation, but has been in various ways employed, by means of wire or silk ligatures, or by external pressure, and I therefore lay no claim to originality in this part of the operation.

The employment of prepared catgut as a suture in operations on hernial sacs was referred to by Mr Lister[2] in his well-known address on Surgery to the British Medical Association at Plymouth, in August 1871; and in connexion with this subject he gave an account of two very interesting and successful cases of irreducible ventral and umbilical hernia, which he had treated by laying open the sac, separating adhesions, reducing the contents, and stitching together with catgut the margins of the openings through which the protrusions had come.

I have myself on several occasions successfully used catgut, and ligatured with it the neck of the sac, in cases of strangulated hernia, after dividing the constricting agent and reducing the contents, with the hope that this proceeding would lessen the risk of a return of the disease.

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[1] *Loc. cit.*

[2] British Medical Journal, 1871, vol. ii.

In connexion with the ligature of the neck of a hernial sac, it may be objected to that the operation might be followed by strangulation and sloughing of the sac, and cases are on record in which these results have followed the employment of a silk ligature; but I am of opinion that the use of catgut, with precautions not to tie the ligature too tightly, will prevent any risk of such an occurrence.

The making use of an inguinal sac to plug the femoral canal, as was done in this case, is, as far as I am aware, an original idea; but the experience of this case would rather tend to prove that such plugging is not so efficacious as ligature of the neck of the sac, for while the inguinal hernia remains cured, the femoral one already shows signs of returning.

Another little point of originality in my operation was the situation and direction of the incision. This incision is one which gives free access to the neck of a femoral sac, close to the general peritoneum, and it was found to be a most efficient one also for exposing the inguinal sacs at the point which seemed best for the application of the ligature.

Third, I would refer to this case as an example of successful antiseptic treatment.

There can, I think, be no doubt as to the value of the catgut ligature or suture in such an operation. A ligature or suture of this material is non-irritating, and may be left buried in the wound with the certainty that it will not require to be interfered with, but that it will become absorbed or otherwise disappear. One objection to the catgut as usually prepared is, that it may dissolve or disappear too soon; but Mr Lister has now, I understand, a new method of preparing this substance, which allows it to remain in the tissues unaltered for almost any length of time. The use of this new catgut must therefore prove invaluable in this and other operations in which a more permanent ligature or suture is desired.

In conclusion, I have not called this case one of "radical cure," because experience has shown that the results of such operations are not often strictly radical. As I have already stated, this or similar means may be usefully employed to assist the application of mechanical support; and if such assistance should be required in any case of reducible hernia, I am inclined to advocate the ligature of the neck of the hernial sac with catgut in preference to the other methods usually employed.

ARTICLE IV.—*On the Reflex Functions of the Spinal Cord.* By WILLIAM STIRLING, D.Sc., M.D., Demonstrator of Practical Physiology in the University of Edinburgh.

(*Continued from page* 919.)

LASTLY, Goltz,[1] after a very extended series of observations, succeeded in showing that "the pithed frog is nothing but a complex of simple reflex mechanisms." He distinguishes three sorts of movements—

1. "Simple reflex movements" are those where the motor action resulting from the application of a stimulus has always the same form.
2. "Reply-movements." They also result directly from a stimulus; the motor actions have no constant form, but have always a distinct purpose corresponding to the disposable force.
3. "Voluntary or spontaneous movements" are those which take place without any direct antecedent cause being detected.[2]

The "automatic movements" he regards as "simple reflex movements," and they therefore belong to the first group. The limits of these groups are not well defined, as occurs everywhere in the division of the organic processes. On the other hand, the author has succeeded in finding a series of important signs by which one may ascertain which parts of the central nervous system are present in the frog. The proof of the existence of the brain is to test whether the animal, on being placed on its back turns again on its belly. If it makes no attempt at this, the section lies behind the medulla oblongata. If the animal turns on its belly, and moves without any appreciable cause (voluntarily), the whole brain has not been divided. If the animal remains passive, and takes no food, but can retain its equilibrium on an inclined plane (*e.g.*, on the inclined edge of a board), it possesses the corpora quadrigemina intact. Then the "Quakversuch" will also succeed. If it does not do this, then the section has injured or divided the corpora quadrigemina. If it springs out of a bath of a concentrated solution of salt as soon as it has been put in, then the section lies in front of the cerebellum. If it remains therein, it has lost the entire centre for progression, and the section must lie behind the cerebellum. If it embraces blindly the extended finger, then the whole brain proper has been removed.

Goltz[3] cannot ascribe consciousness, as meant by Pflüger, to the spinal cord; and is also convinced that the spinal cord gives no indication from which one may conclude what deserves the name consciousness. We must assume that there exists in the spinal cord a simple reflex centre, whose function consists in replying by a contraction of the flexor muscles to a stimulus produced by extension of the posterior extremities. The ordinary movements made by a decapitated frog to remove any stimulus from an irritated portion of skin—generally with the leg on the side to which the irritant was applied, and using the other limb when the leg stimulated is removed, or so fixed that it cannot be used—may be explained by mechanical arrangements, for the sound limb may be used without the one stimulated having been cut off at all. As to whether a brainless frog also possesses sensation, nothing definite can be cited.

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[1] Beiträge zur Lehre von d. Functionen d. Nervencentren d. Frosches, p. 91. Berlin, 1869.

[2] *Loc. cit.*, p. 92.

[3] *Loc. cit.*, p. 100.

Pflüger believes that no sensory condition is fixed by the spinal cord, *i.e.*, no recollections are retained by it, so that, at the moment when cerebral consciousness returns, no impressions previously received can be communicated from the spinal cord to the brain. In relation to this point, Wundt[1] defines the difference between the action of a self-regulating mechanism and the will, and also the difference between a capacity which I may name the susceptibility for stimuli and sensation. The will alone, he remarks, is sufficient to indicate with almost absolute certainty the painful part; but with regard to its exact position, we perhaps have no precise knowledge, but probably obtain the information by investigating it through our own sense of touch and vision.

The voluntary use of our motor organs, and the conscious reaction to external stimuli, would be enormously increased if, in each individual case, it was necessary that we should have a clear conception of the amount of movement to be executed and of the seat of the sensation. We can, therefore, only assume that the will uses a mechanism which works accurately, and to which we only require to give the first impulse in order to obtain an exact fulfilment of our orders with relation to all existing circumstances. As the will is only an internal stimulus, which, after it has given the first impulse to movement, leaves the further course to the self-regulating physiological mechanism, so when the latter is set in action by any external stimulus, of course there is an accommodation to external circumstances without a conscious sensation of the stimulus being necessary. Secondly, in the condition of the pithed animal, the essential signs by which we judge of the presence of consciousness are wanting. No single movement takes place spontaneously. In the soul of such an animal the impressions cannot last for any appreciable period beyond the time of their action.

Momentary sensations without any connexion cannot be ascribed to consciousness, inasmuch as the latter represents a perfect connexion of internal conditions. The internal condition of any organism that replies to a stimulus by a momentary movement without any further result, we are no more entitled to call sensation or conception, than the internal condition of all matter.

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[1] Grundzüg d. Physiolog. Psychologie, vol. ii. p. 828. Leipzig, 1874.

Goltz[1] makes the following pregnant remarks on the sensory endowments of an animal without a brain:—"Sensation is a purely subjective phenomenon. It can only be felt by the animal which is the subject of it. Another person can only see the movements which have been caused by the sensory processes. If an animal makes no movements, although capable of doing so, under circumstances which appear well suited to give it violent pain, one can at least say that it is improbable that it has sensation."

The above conclusion is drawn by Goltz from the following ingenious experiments, which have been confirmed by M. Foster.[2] A brainless and a blinded frog were placed in a vessel whose bottom was covered with water which was gradually heated from without. When the temperature rises to 25° C. the frog with the brain begins to be restless; it respires more rapidly, and, lastly, seeks to escape from the hot bath, until at about 42° it dies, exhibiting signs of pain and tetanic spasms. In the meantime the brainless frog remains motionless, until at last the heat-tetanus (coagulation of the myosin) and death occur. If a second frog, whose brain has been removed, is suddenly thrown into the heated water, it is at once thrown into violent spasms, and dies just like the frog with the brain. This experiment shows very clearly how the mechanism of the spinal cord, following the general law of nervous excitation, only reacts to such stimuli as act with a certain rapidity; whilst a gradually-increasing stimulus is completely without effect. In the brainless frog only this law of excitation appears. It gives no sign whatever that the heat causes it pain.

Whoever after this would wish to ascribe sensation to the nerves, or to the remainder of the spinal cord, must make the paradoxical assumption that the capacity for feeling remained in the sensory parts, but that they do not possess the power of transferring their sensations to the equally uninjured motor parts. I do not know how we can formulate our knowledge on this subject better than in the words of Ludwig,[3] written in 1858:—"If under the term soul, as this word is usually employed, we include the process by which sensation, consciousness, and memory are produced, it can be shown from the phenomena which are known to take place in the human spinal cord when it is separated from the brain, that no such soul is present in the former. But as the purposiveness of the movement which occurs gives us no cause to assume the existence of something analogous to a conscious soul, and on the other hand the whole reflex problem can be sufficiently explained as consisting of molecular movements, or by the transformation of movement and potential energy, etc.—or, in other words, on mechanical grounds— it seems advisable that the many-sided expression soul should be dropped altogether, and instead of this, the phenomena of reflex movement ought to be carefully observed under the greatest variety of conditions possible."

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[1] *Loc. cit.*, p. 127.

[2] Journal of Anatomy and Physiology, viii. p. 36.

[3] Lehrbuch d. Physiol. d. Menschen, Bd. I. 2d edition, Leipzig, p. 181.

To this programme we are indebted to Ludwig's school for a series of communications upon—

1. The channels of reflex irritation; sometimes followed with relation to the so-called vaso-motor nerves.[1]

2. A topography of reflex movements is also given, *i.e.*, the dependence of different kinds of movements on the part of the skin of the frog's leg stimulated, was investigated (*Sanders-Ezn*).

3. The dependence of the duration of latent stimulation on the strength of a (chemical) continually acting irritant, was also demonstrated (*Baxt*).

But in other places also the interest of the subject had excited physiologists to investigate in different directions the remarkable process of reflex action.

A large number of physiologists have attempted to limit the place in the spinal cord, where the transference of the sensory impression into movement takes place. It will, however, be sufficient for me here to cite the authors, and refer to their works for the details.[2]

[1] Arbeiten aus d. physiol. Anstalt zu Leipzig by Loven, Cyon and Ludwig, Asp. Dittmar. Miescher, Owsjannikow, Nawrocki, Woroschiloff.

[2] Calmeil, l. c., 1828.—Legallois Œuvres, 1830, p. 136.—Johannes Müller, Bestättigung des Bell 'schen Lehrsatzes, printed in Romberg's Uebersetzung von Karl Bell's phys. und pathol. Untersuchungen, Berlin, 1832.—J. Müller, Untersuch an Salamandra Maculata, 1830, cited in his Handbuch d. Physiologie, 1844. Bd. I., p. 611 and 612, Baly's translation, p. 757.—Marshall Hall, l. c , 1837.—Grainger, l. c , 1837, p. 63.—Volkmann. Müller's Arch., 1838, p. 115.—Stilling, Versuche über die Spinàlirritation, 1840, p. 314.—Kürschner, l. c., 1840, p. 169.—Van Deen, Traité et découvertes sur la physiologie de la moêlle épinière, 1841.—Flourens, l. c., 1842.—Longet, l. c., 1842, p. 317.—Volkmann, 1844, Nerven-physiologie, l. c., p. 543.—Eigenbrodt. Ueber die Leitungs Gesetze im Rückenmarke, Giessen, 1849.—Türk, Sitzungsb. d. Wiener Akad. d. Wissensch., 1850.—Brown-Séquard. De la transmission des impressions sensitives, Comptes Rendus, 1850.—Pflüger, 1853, l. c., p. 62.—Schiff, Physiologie, 1853, p. 206 —Schrœder van der Kolk, Anatom. phys., Onderzoek. over het Lamenstel., en de Werking van het Ruggemmerg, Amsterdam, 1854.—New Sydenham Society series translation, 1859.—Auerbach, 1856, l. c., v. Bezold, Über die gekreuzten Wirkungen des Rückenmarks, 1858, Zeitschrift f. Wissench. Zoologie, Bd. 9, p. 305.—Chauveau, Journal de la Physiol., tome i. p. 176, 1858.—Brown-Séquard. Lectures on the Phys. and Pathol. of the Central Nervous System, London, 1860.—Cl. Bernard, Récherches Exper. sur les Nerfs vasculaires, ect. Comptes Rendus, 1862.—Cayrade. Récherches crit. et exper. sur les Mouvements reflexes, Paris, 1864.—Schelske, Reichert & du Bois' Arch., 1864, p. 151.—Set-chenow, Ueber die Nervenbahnen Welche die vordern und hintern Extremitäten des Frosches untereinander verbinden, 1865, p. 817.—Sanders, Geleidungs banen in het Ruggemmerg, Groningen, 1866.—Vulpian, Leçons sur la Physiol. du Système Nerveux. Paris, 1866.—Westphal. über Erkrankung d. Rückenmarks, 1866.—Virch. Arch., Bd. 39.—Eckhard, Expt. physiol. d. Nervensystem, p. 244, Giessen. 1867.—Masius & van Lair, Von d. Lage and Ausdehunng d. Reflexcentren beim

With regard to the conduction of impressions, it results from the total of the above cited works that the motor and sensory paths in the spinal cord follow a cross direction; and that the gray substance surrounding the spinal canal can conduct impressions in every direction. The recent investigations of Woroschiloff[1] have limited with special precision the course of the motor and sensory path in the lumbar portion of the spinal cord in the rabbit. The physiological effects of partial exactly graduated sections of the individual columns of the spinal cord were tested, and then the extent of the injury was ascertained by making a microscopic examination of the part. The results of these investigations show—

After division of the anterior and posterior columns, and nearly the whole of the gray substance, no disturbance of the transmission of motor or sensitive impressions through the cord was detected. The animal sits and moves in a perfectly normal manner.

Section of both lateral columns entirely prevents the transmission of impressions through that region of the cord. Irritation of one hind-leg causes reflex movements of the same leg, or, if the irritation be a strong one, of the opposite leg also. Irritation of a fore-leg causes movement in the anterior, but not in the posterior, part of the body. Irritation of the cervical cord causes movements in all the muscles of the body except those of the hind-limbs.

Section of the posterior columns alone is absolutely without effect on the condition of the animal. There is nowhere hyperæsthesia nor anæsthesia.

Other modifications of the experiment were made, and the general result showed that the channels of motor and sensory impressions lie in the lateral, and not in the anterior and posterior columns of the cord. This is in opposition to the generally received opinion of physiologists. Many other modifications of the experiment were performed, and the general results may be stated as follows:—

Motor and sensory nerve-fibres are found in all parts of the lateral columns.

Sensory fibres from both hind-limbs are found in each hind lateral column, but the fibres in either column, which come from the leg on the opposite side, are capable of producing stronger reflex movements in the anterior part of the body than are called forth by excitation of the fibres which come from the leg on the same side (crossed hyperæsthesia).

Frosche, Centralblatt, 1870, p. 1. Spiro, Physiol. topogr. Untersuch, Bulletin de l'Academie de St Petersburg, 1870.—Masins et Van Lair, de la Situation et de l'étendu des Centres réflexes de la Môelle Epinière chez la Grénouille, Bruxelles, 1870.—W. Müller, Beitr. z. path. Anat. d. Mensch. Rückenmarks, Jena, 1871. -Leyden, Klinik d. Rückenmarks Krankheiten, 1874.

[1] Ludwig's Arbeiten, viii.

The centripetal fibres whose excitation produces these strong reflex movements, as well as those whose section on the opposite side gives occasion to them, lie in the middle third of the lateral columns, while the anterior and posterior thirds contain sensory fibres which call forth movements of only moderate intensity in the anterior part of the body.

Motor fibres for both legs are found in each lateral column, but the motor fibres in different parts of these columns are called into activity in different ways. The reflex movements due to irritation of a fore-leg can be excited in a hind-limb only when the anterior half of the lateral column on the same side is preserved.

The co-ordinated movements of sitting and springing, and those produced by irritation of the cervical cord, are transmitted to each hind-limb through the middle third of the lateral column on the same side.

The fibres which preside over co-ordinated movements of the hind-limbs, as well as those whose section causes hyperæsthesia on the side of the injury, lie in those parts of the lateral columns which are nearest to the gray substance. The motor fibres of the foot and leg proper seem to lie in the lateral columns externally to those of the thigh.

All authors agree in this, that the individual sensory fibres are by no means so arranged with regard to the motor ones, that, upon stimulating a small cutaneous area, a muscle, or even a group of muscles, contracts, but (as was already shown) that the irritation extends in the spinal cord. Calmeil,[1] in 1828, remarks—"The excitation has a great tendency to be communicated from those of the motor-spinal nerves whose origin is nearest to that of the irritated sensory nerves."

Volkmann[2] showed—"That the extent of the reflex movement depended principally on the strength of the stimulus, and on the degree of the excitability. If a freshly decapitated frog is stimulated by gently touching any part of its body, the movement is often limited to the region of the spot stimulated. Thus, it is sometimes possible, by gently tickling one toe, to produce movements of the foot alone. With a somewhat stronger stimulus, the whole limb is moved, a part only having been touched. Lastly, with still stronger stimuli, the movement extends to all the muscles; and it appears remarkable that comparatively weak stimuli, *e.g.*, slight touching with the hand, is strong enough to produce general movements. The less the irritability, the more limited will be the movement, the intensity of the stimulus remaining the same. Thus, a considerable time after decapitation, it is impossible, by stimulation of one limb, to produce movement in parts other than that stimulated." Analogous remarks are made by Johannes Müller[3]—"Irritation propagated to the spinal cord most readily affects those motor nerves which arise nearest to the roots of the exciting sensory nerves, and the irritation of the cord, and of the motor nerves arising from it, diminishes in proportion with the distance from that point. Compare also the works indicated below.[1]

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[1] *Loc. cit.*, p. 92.

[2] Müller's Archiv., 1838, p. 23.

[3] *Loc. cit.*, p. 619. Baly's translation, vol. i. p. 765.

Fick,[2] on the contrary, has observed identical washing movements by mechanical or chemical stimulation of the skin; whilst, by single induction shocks applied to the larger cutaneous nerves, he could only obtain simple contractions, or contractions of single groups of muscles. By rapid intermittent stimuli, tetanic muscular contractions were obtained, "but no purposive reflex movements." Also, after stimulation of the skin with acids (concentrated), reflex movements in the form of contractions occur.

Chemical stimuli produce in the frog, almost without exception, tetanic contractions; whilst, by mechanical stimulation, as a rule, repeated contractions, and seldom a short tetanus, are produced.[3] Up to the present time, only a few experiments have been performed concerning the relation of a reflex muscular contraction to the strength of the stimulus.

Wundt[4] has found that, with an increase of instantaneous stimuli applied to the nerve roots of a frog, the height of the curve drawn by the muscles supplied by these nerves increased: that from a more distant part of the central end of the nerve, stimulation is less active. When the frog is poisoned with strychnia, then the contraction, even with the weakest stimuli, is higher than under normal conditions. At the height of the action of the poison, the reflex tetanus shows scarcely any difference, whether the strongest or the weakest stimuli are applied.

An experiment—a seemingly paradoxical case—from Setschenow[5] belongs here. This experimenter remarked that very strong intermittent induction shocks, applied to the central end of the sciatic nerve, did not at once occasion reflex movements as weaker stimuli do; but when Wagner's hammer is held fast, *i.e.*, when the stimulus is interrupted, or after the stimulation has lasted for a long time, the animal springs away. Similar reflex movements have been observed by other physiologists.[1] Goltz[2] found that the heart of the frog stood still when one tapped on its abdomen; and later,[3] he remarks that the *Klopfversuch* does not bring the heart to a standstill when at the same time the skin of the legs is strongly stimulated. The cessation in the action of the heart which is produced by stimulation of the splanchnics[4] cannot be set aside by stimulation of the skin. Nothnagel[5] remarks that strong electrical stimulation of sensory nerves diminishes the reflex activity; and Goltz[6] has shown that the reflex croaking which is produced by a brainless frog when its back is gently pressed upon, is inhibited by other sensory stimuli. Lewisson[7] made similar experiments on the inhibition of the activity of motor nerve centres by stimulation of sensory nerves. After placing caoutchouc rings round both the arms, the reflex movements were absent when the cutaneous nerves were strongly pinched or stimulated by electrical currents. The inhibition did not occur when the posterior roots of the spinal nerves were divided at the height of the brachial nerves; also, in such animals, he succeeded in inhibiting other reflex movements by strong stimulation of sensory nerves.

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[1] Meihuizen, Über d. Einfluss einiger Substanzen auf die Reflex Erregbarkeit, Pflüger's Arch., 1873, Bd. vii. p. 215, says, "The central result of chemical stimulations is limited to the local centre for the extremity stimulated, whilst the central result of a mechanical stimulus extends to all reflex centres!"—Pflüger, *loc. cit.*, p. 62.—Sanders-Ezn., Vorarbeit f. d. Erforschung d. Reflex-Mechan. im Lendenmarke d. Forches, Ludwig's Arbeiten, 1867, p. 6.—Setschenow, Die Elektrische u. Chem. Reizung ect., p. 25, Graz, 1868.—Goltz, 1869, *loc. cit.*, p. 120.—Foster, Handbook for the Physiol. Laboratory, 1873, p. 407.

[2] Einige Bemerkungen über Reflexbewegungen, Pflüg. Arch., 1870, Bd. iii. p. 331.

[3] Sanders-Ezn. Arb. aus d. Physiol. Anstalt zu, p. 28. Leipzig, 1867.

[4] Grundzüge der Phys. Psychologie, p. 261. Leipzig, 1874.

[5] *Loc. cit.*, p. 23.

In the older papers upon reflex action, the time which elapsed between the application of a stimulus to a sensory nerve and the beginning of the resulting movement remained quite unnoticed.

Prochaska[8] thought that the nerves "took up the external and internal impressions of the stimuli and conducted them with a lightning-like speed to their destination." Even Joh. Müller, who was acquainted with the peculiarity of the "individual error," like Bessel, ascribed the difference in time which elapses between the perception of an impression upon the eye (by observing an audible pendulum) and the marking of the same, to this, that we cannot perceive two sensations simultaneously (through eye and ear). Müller adds, "The time required for the propagation of a sensation from the surface of the brain and spinal marrow, and for the reflected action producing contractions of the muscles, is immeasurably short. Frogs poisoned with opium or nux vomica are, at first, in a state of such excessive sensibility that the slightest touch on the skin excites convulsions of the whole body. Here the impression made on the skin is propagated first to the spinal cord, and from the cord to all the muscles, and, nevertheless, I have been unable to detect the slightest interval between the moment when the skin was touched and the occurrence of the muscular spasms."[1]

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[1] Herzen, Expériences sur les Centres Modérateurs de l'Action Reflex. Turin, 1864.

[2] Virchow's Arch., 1862.

[3] Centralblatt, 1868, p. 593.

[4] Bernstein, Centralblatt, 1863, No. 52.

[5] Bewegungs Shemmende Mechanismen im Rückenmarke d. Frosches, Centralblatt, 1869, No. 14.

[6] Centralblatt, 1865, p. 706.

[7] Reichent und du Bois Arch., 1869, pp. 255–266.

[8] Physiologie d. Menschen, 1840, Bd. i. p. 136.

In the fundamental treatise of Edward Weber, "Muskelbewegung,"[2] there is the first notice, as far as I am aware, of the pause which exists between the application of the stimulus and the reflex movement. He records that in a beheaded frog, one of whose sciatic nerves was isolated and stimulated with the electro-magnetic rotation apparatus, that "the movements did not occur at the same moment in which the galvanic current began, as is the case when the spinal cord or nerves were stimulated directly, but nevertheless, in spite of the powerful action of the apparatus, a considerable time always elapsed before the movements followed."

The first measurements of this time "of latent stimulation" we owe to Helmholtz.[3] The time necessary for the conveyance of an impression from the sensory to the motor root in the spinal cord he estimated at $\frac{1}{30}$ to $\frac{1}{10}$ of a second and more, *i.e.*, twelve times as long as the time necessary for the excitation to pass along the sensory and motor nerves. The duration is specially long when the frog, poisoned with strychnine, possesses a high reflex excitability.

S. Exner[4] "measured the sensory conduction in the spinal cord in man." He reckons the rapidity of propagation in the spinal cord (taking the rapidity of propagation in the nerves at 62 metres per second) at 8 metres per second. The "time of reaction ('Reactionszeit') increases with fatigue, diminishes with increasing intensity of the stimulus and in consequence of exercise."

Wundt[5] also found that the time of latent stimulation diminished with increase in the strength of the current. At the same time he remarks (what Volkmann and Weber had concluded from simple observation) that the curve of the reflex contraction is of much longer duration than that which is obtained by direct stimulation. After poisoning by strychnine the minimal contraction is also longer than the one obtained under normal conditions. "With increased action of the poison it very soon passes into a tetanic contraction, so that the time of latent stimulation can be increased to more than twice its usual duration. Sometimes the differences in the time of latent stimulation with strong and weak stimuli increase enormously."

Rosenthal[6] found that the reflex time ("Reflexzeit"), *i.e.*, "the time necessary for the transference of a sensory stimulus to a motor-nerve," on stimulating (with simple electrical shocks?) the intact skin or the exposed nerves, was dependent on the strength of the stimulus, "so that with very strong stimuli it may become imperceptibly small." He did not employ such stimuli as gave the maximum of reflex action. "The time also of transverse conduction ('Querleitung')—*i.e.*, the difference in time between the reflex time from one point on the skin to a muscle situated on the same side, and the reflex time from the corresponding point on the skin to the muscle of the opposite side—with sufficient stimuli reaches a maximum, which becomes smaller with over-maximal stimuli, and quite inappreciable with very strong ones." Both latent times increase with the fatigue. "The reflex time is greater for a point further removed from the spinal cord, than for a nearer one; with strong stimuli the difference is small." S. Exner[1] also found, that the time of reaction like the reflex time (closure of the eyelid after electrical stimulation of the cornea) with strong stimuli is smaller than with weaker stimuli (0·05—0·06 secs.).

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[1] Joh. Müller, Handbuch der Physiologie d. Menschen (Elements of Physiology), 1843, Bd. i. p. 583. Translated by W. Baly, vol. i. p. 731.

[2] Wagner's Handwörterbuch d. Physiologie, Bd. iii. Abth. ii. 19.

[3] Berichte d. k. Akademie d. Wissensch. zu Berlin, 1854, p. 328.

[4] Die experimentelle Untersuchung der einfachsten psych. Processe, Pflüg. Arch., 1873, Bd. vii. 632 and 638.

[5] Physiologische Psychologie, Leipzig, 1874, p. 262.

[6] Berichte der Akademie der Wissenschaften zu Berlin, 1873.

Schiff[2] asserts that "the time which is necessary for the occurrence of reflex movements is longer, the more the thickness of the gray substance at one point which lies between the point stimulated and the part to be moved is diminished by incisions into the spinal cord (only tried at one point in the length of the spinal cord in *frogs*)."

When several single stimuli are applied to sensory nerves with a certain frequency their effects become summated in the organ which produces the reflex act. Methodical investigations upon this point have up to this time been wanting. Of the investigations upon this subject those of Setschenow are the most extensive. In his monograph "Ueber die elektrische und chemische Reizung der sensiblen Rückenmarknerven des Frosches,"[3] he gives as the result of one series of observations, "The reflex and locomotor centra possess in a high degree the property of summing up individual impulses communicated to them" (p. 25). The central end of the sciatic nerve of a frog was stimulated by the closing and opening of a constant current. "The limit of the summation of the effects of single shocks in the time (*i.e.*, how often shocks of a given strength and direction must succeed each other in order that a summation of their effects may take place) could not be estimated by my apparatus" (p. 11).

He employed frogs from which only the cerebral hemispheres were removed, and cites one or two experiments; but remarks that "these experiments must be further investigated." "Increase of the current or increase in the number of interruptions causes the result under similar conditions to follow more quickly" (p. 12). By means of induction-shocks also, each single shock having very little effect, very considerable effects can be obtained when the Wagner's hammer is allowed to vibrate.[1] A similar increase of the effect with the strength and duration of the stimulus as has been shown for electrical stimulation, has already been shown (Türck[2]) to be true for chemical (dilute sulphuric acid) stimulation, and this point has been methodically investigated by W. Baxt.[3] The latent effect of very dilute acid (0·0006 per cent.) may last even as long as two minutes. Baxt found that the times of action (from the dipping of the limb into the dilute acid until it was again withdrawn) increased in a geometrical progression, whilst the degree of acidity followed an arithmetical one (p. 86). "The stimulating action of the acid, therefore, does not depend alone upon the quantity which penetrates to the nerve, but also upon the rapidity with which the diffusion occurs. While the quantity of acid which penetrates to the nerve is equal to the product of the density into the time of diffusion, and while with its diminishing density, the time of diffusion necessary for a contraction increases more rapidly than the acid has diminished in density, it therefore follows, that for the production of a contraction, more acid must have passed in, the slower it has penetrated."

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[1] Pflüg. Arch., 1874, p. 531.

[2] Lehrbuch d. Physiologie, Jahr 1858–59, p. 228.

[3] Graz, Universitäts-Buchhandlung, 1868.

This condition is easily explained if we assume that each particle of acid coming to the nerve produces therein a periodic process, a kind of momentary impulse, *e.g.*, of induction-shock. According to this view of the question, what causes the stimulation is not the number of particles of acid present in the nerve, but the number of particles of acid which penetrate to it in the unit of time ; or otherwise expressed, it is not the condition remaining after the entrance of the acid which produces the excitation, but only the act of decomposition, or the changes in the nerve-mass. "The possibility of producing contractions would come to the acid when its masses followed more rapidly than the course of the nervous excitations, so that each successive particle of acid strengthened the movement induced in the nerve-mass by its predecessor."

I tested this view by means of analytical electrical experiments on *Rana esculenta.* The brain and spinal cord were destroyed as far down as the origin of the brachial plexus; and, to prevent bleeding, the upper part of the canal was plugged. The part stimulated was the peripheral termination of the nerve in the skin of one foot. The stimuli employed were induction-shocks from a du Bois Raymond's induction machine, the opening and closing shocks being rendered, by special arrangements, as nearly equal in intensity as possible. A series of single stimuli was applied to the skin at certain intervals; each stimulus of itself being unable to produce a reflex action, so that when these stimuli were applied with a certain frequency, their effects would be summated and a reflex action result. The differences in the latent time dependent upon variations in the strength of the stimulus, its frequency and nature, with fatigue, etc., are the points to which I specially directed my attention in this investigation, and found that the greatest number of stimuli of minimal strength obtainable from a du Bois inductorium, provided with a Wagner's hammer, acted after a very short latent period, and soon lost their effect, if at least half a minute of rest was not given between the periods of stimulation. Increased currents produced reflex movements after very short latency. If such discharges, however, were repeatedly produced at intervals of five to ten seconds, the time of latent stimulation increased to three and, exceptionally, to five seconds. The duration of the latency could not be essentially diminished by strengthening the frequent stimuli. No great difference in the latency was observed, whether the stimuli were strong or weak, on applying forty-eight induction-shocks per second to the limb of the frog. The size of the contraction, however, increased with the strength of the current; but the latency only when fatigue set in. The latency therefore appears to be independent of the intensity of frequent ($\frac{1}{50}''$— $\frac{1}{18}''$) stimuli. The excitability of course varies with each individual frog; but I found that artificial inspiration in the frog had no effect on the reflex processes. Probably this may be explained by the large cutaneous inspiration in this animal.

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[1] P. 14. Compare Fick, Pflüg. Arch., 1873, Bd. iii. p. 329 ; Wundt, l. c., p. 262.

[2] Zeitschrift der Ges. d. Aertze, Wien, 1850, Heft iii.

[3] Arbeiten aus d. phys. Anstalt zu Leipzig, 1871.

Next, the effect of stimuli of medium frequency ($\frac{1}{2}''$— $\frac{1}{10}''$— $\frac{1}{15}''$), the strength of the stimulus being varied, was investigated. With these intervals between the stimuli, the duration of the latent stimulation diminished when the sub-maximal stimuli were strengthened. The latent times decrease with increase of the strength of the stimulus. The time of latent stimulation, however, is not by any means a mere function of the strength of the current, as is to be observed in the height of the contraction of a muscle on stimulating its motor nerve. When the closing and opening induction-shocks were rendered as nearly equal in intensity as possible, the duration of the latency of a freshly prepared frog is not essentially lengthened when the strength of the stimulus is diminished. Further, as the result of a large number of experiments, we may say, that, by varying the intensity of the individual shocks, the latent time can only be changed within narrow limits; by varying the interval of stimulation, on the contrary, we possess a means of producing exceedingly great variations in the duration of the latent period. But still the latency cannot be indefinitely varied by changes in the interval of stimulation. Stimuli applied at long intervals (1·5″) require strong currents which soon extinguish the excitability. Intensely strong stimuli (primary coil pushed over the secondary) applied at intervals of 2″ to 2·5″ act deleteriously even though they do not produce any reflex action. They tend to produce two different results—viz., to excite motion and extinguish life. The injurious property seems to belong in a high degree only to the intensely strong stimuli; movement being best excited by repetition of the shocks rather than by an increase in their strength. The injurious effect therefore is more obvious with stimuli applied at long intervals (necessitating, as they do, strong currents to be effective at all) than with stimuli following each other at short intervals.

Violent reflex spasms last longer than the stimulus, and this sometimes in a very pronounced manner. The vibrations in the nerve elements of the spinal cord which are produced by single induction shocks do not long retain their intensity, and remain local. Hence why it is so difficult to increase the effect of stimuli applied at long intervals (2″—2·5″), so that they may produce extensive reflex movements; whilst frequent stimuli ($\frac{1}{4}$″) of only moderate strength may cause the frog preparation to exhibit those most complicated purposive and washing movements which are so pronounced with chemical stimuli. I am of opinion that every stimulus causes an increase in the movements of the reflex centres; that the effects of several single shocks sum themselves up until sometimes a preliminary reflex action is produced; that then the summation goes further, until the final contraction, or a spasmodic attack, terminates the process. There is this important point, however, that further movement is not prevented by every small reflex contraction, but only by a strong nervous discharge. It is therefore not so much the strength or extent of the reflex contraction which is of fundamental importance, but the circumstance whether or not a complete discharge has taken place. It is as if there was a store of energy, and this was exhausted by the production of a simple or complex reflex action. In fresh preparations this store can again be quickly replenished.

We may perhaps make the process clearer by a simple comparison. A pendulum requires minimal impulses to be raised a little from the position of rest; returning, however, it is only able to overcome very small resistances. With a greater swing it gains on the return also greater velocity, and is able, with corresponding mass, to convey a larger amount of kinetic energy to a resisting body. If the resistance is not great, it expends the remainder of its energy in rising in the opposite direction; on its return it can again overcome obstacles; and so on until its energy is used up, its vibrations from the point of equilibrium become invisible, and it at last stands still. Let us further imagine a second pendulum of the same length, and so placed with regard to the first one that the planes of vibration of both are parallel. An easily movable process (*e.g.*, a piece of watch-spring with little elasticity) is fixed to the second pendulum, so that the first pendulum in passing the position of rest strikes against it. With small excursions the spring will receive a feeble shock, not sufficient to bend it. The process conveys the shock to the second pendulum.

This moves more feebly than the first, which was hindered by the resistance, but not brought to a standstill.

With greater elongation the first pendulum falls with greater velocity against the spring-process, presses it aside, and completes the other quarter of its vibration. In the meantime, however, the second pendulum has taken up the part of the force for which the spring was still inflexible. It makes a small vibration, but as its duration of vibration is equal to that of the first, also a quarter vibration, it arrives simultaneously with the strongly-moved one at the position of equilibrium. There it receives from the rapidly-falling one a new impulse, which the spring again partly conveys to the second one. In this way the impulses of the first pendulum become summated in their action on the second, and at last it may acquire the complete amplitude of the first one.

The application of this picture to the reflex process is simple. We require to conceive that the nerve-motion of the sensory nerves impinges upon the first pendulum, that the next motor nerve receives an impulse from the pendulum, as soon as this on its return passes the position of rest. From the secondarily moved pendulum the ends of other nerves are set in motion, as soon as the kinetic energy of the shock has exceeded a certain limit. A third similarly coupled pendulum would represent a vibrating complex arrangement of the third order. To explain the increased consumption of material, arising from secondary and tertiary discharges, one must introduce a certain complication into our picture, that at the moment where the forces of the secondary, or else of the tertiary pendulum, have become sufficiently increased to introduce a movement in the ends of the nerves, a sort of explosion takes place there, which sends the motor impulse towards the periphery, but also reacts upon the centre. The production of heat and the production of acid may be partly the cause of the temporary diminution of the central excitability. By violent discharges the whole system becomes more difficult to move, hence the conveyance from one vibrating system to another must last very long before the force sufficient for a discharge reaches the second or third system.

But the exhaustion of the central parts need not necessarily be blamed for the inaction of the cutaneous stimulation. The peripheral nerves may refuse to act, and render the bridge to the centre impassable to the electrical impulse.

This is easily ascertained by comparing the reflex excitability of the limb experimented on with that of the other.

The next point of importance which attracted my attention, and it is a fundamental one, is whether a reflex action could be produced without the summation of stimuli. This at first sight seems unquestionable. A prick of the skin causes an animal to fly away; pressure on the foot of the frog causes lively contractions; and the very short discharge of a Leyden jar has been found by Cavendish and Volta capable of producing a reflex action both in fishes and man. I have myself observed that, with very strong stimuli applied at long intervals to a fresh preparation, the reflex action occurred sooner than the second shock. It is agreed by all experimenters that incomparably greater strength of stimulus is required to produce reflex excitation by single induction shocks than for the stimulation of motor nerves, whilst for summation reflexes comparatively weak stimuli are sufficient.

The effects of single induction shocks were then tested and compared with those of repeated stimuli. A reflex action was easily produced by a single induction shock when the shock was very strong. The closing shock, however, is only exceptionally effective in the production of a reflex action, and it is always surpassed by the opening one.

From the fact that in the opening induction shock strong oscillating discharges have been proved to exist by Donders and Helmholtz,[1] we are led to the idea that the reflex contractions produced by it are also to be regarded as the effects of summation. I found that a single induction shock of great intensity gave a reflex action after a long latent period (1·7″), and produced no further effect; while tetanizing stimuli of only moderate strength caused a reflex contraction after an exceedingly short period of latency. Here was a curious point—how one single strong shock should require a longer time for propagation than many weak stimuli. These apparently single stimuli must also be brought into the category of summated ones; and we may therefore say that reflex movements can only be liberated by repeated impulses transmitted to the nervous centres.

I may formulate my results shortly thus:—

1. Comparatively strong single stimuli applied to the skin of a frog's limb can sum up their effects even with an interval of 2″ between the shocks. The time of latent stimulation often reaches the value of 90″ or more.

2. The time of latent stimulation increases with the interval of stimulation, *e.g.*, the intensity of current being constant, in one case with an interval of $\frac{2}{5}$″, the latent period was 30″; with $\frac{1}{5}$″, only 5″.

3. The time of latent stimulation is shorter with strong stimuli than with weak ones; and the same is true for different directions (ascending and descending) of equally effective stimuli.

4. Stimuli following each other rapidly (such as can tetanize a muscle) always produce in a fresh preparation a reflex action. If the intensity of the exciting current is diminished, the duration of the latent stimulation becomes only slightly lengthened; while

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[1] Donders' Proces-verbaal van de Academie te Amsterdam, 1868. 30 Mai, No. 1. Helmholtz Verhandl. des Naturhist. Vereins zu Heidelberg, 1869. Both referred to in Wiedemann's Lehre vom Galvanismus und Elektromagnetismus. 2 Auflage, Braunschweig, 1874. Bd. ii. Abth. 2, pp. 360 and 128.

the reflex contraction, on the contrary, become smaller, and at last disappears.

5. The time of latent stimulation increases with fatigue. Still, ineffective stimuli are often followed by an increase of the excitability.

6. The summation can proceed after a small discharge (small contraction), and render stronger discharges possible.

7. Stimuli of moderate frequency can be compensated by a simultaneous change in the frequency and intensity of the stimulus, but these must be changed in opposite directions.

8. Simple induction shocks of great intensity, applied to the skin of the frog, produce reflex movements in the same limb after a latent period of 0·1″ to 2″, exceptionally after 3″.

9. The duration of the latent stimulation rapidly increases with the fatigue, and soon the preparation ceases to react to single (opening or closing) shocks.

10. The duration of the latent period in many cases is independent of the strength of the exciting induction current; very strong opening induction shocks often act after short latency.

11. Apparently single stimuli (opening induction shocks) can be brought into the category of summated stimuli.

ARTICLE V.—*Notes on an Interesting Case of Pneumo-thorax.*

By HENRY M. CHURCH, M.D., B.Sc.

(*Read before the Medico-Chirurgical Society of Edinburgh, 5th April.*)

ON the morning of 10th February, at 10 A.M., I was asked to go at once and see a man who had gone out to his work in his usual health, but had taken suddenly ill and had been brought home in a dying state.

On my arrival I found J. B., aged 37, by occupation a hammerman, lying on his back in bed in a state of collapse, his features pale and anxious, his eyes staring, and a cold perspiration on his forehead. He complained of a very severe pain in the right side of chest, aggravated on movement, and extreme difficulty in breathing, or, to use his own expression, "he felt that he was suffocated, and could neither breathe in nor out." His pulse was small, rapid, and irregular. He had a short and frequent cough, and could scarcely speak, so that I had to obtain much of the necessary information from his attendants.

Upon exposing the chest, it presented a fixed appearance, the right side being perfectly motionless, and the respiratory movements of the left very much impaired. The right appeared enormously distended, the ribs as widely separated as possible, and the intercostal parts much stretched.

On percussion, the note was highly tympanitic all over the area of the right lung, except posteriorly, where there was slight dulness. The tympanitic note extended considerably to the left of the sternum. On auscultation, anteriorly nothing whatever could be heard; posteriorly vocal resonance was increased, and there was slight ægophony. Vocal fremitus was absent.

On left side the respiratory sounds were rapid and puerile.

The heart and pericardium were much displaced downwards and to the left side, the apex beat being distinctly seen between four and five inches below the left nipple in the mammillary line. The severity of the pain which the patient suffered, and the distressing dyspnœa, rendered immediate action necessary, and deterred me from taking accurate measurements or further notes of his condition.

Having made necessary antiseptic arrangements, I introduced a small trochar and canula, under the antiseptic spray, into the right pleural cavity, between the fifth and sixth ribs near their angles. This procedure, which allowed a quantity of air to escape with a hissing sound, was followed by almost immediate relief. I found that by placing my finger over the canula, and only removing it during the expiratory part of each cough which I asked the patient to make, the air was by degrees expelled, the heart returning nearly to its normal position, and the breathing becoming quite easy; the lung on the sound side, no longer encroached upon by the displaced mediastinum, expanding naturally.

The canula was now withdrawn, a piece of sticking-plaster placed over the puncture in the skin, and a flannel bandage applied to the chest.

Upon inquiring into his history I found that seven years ago he had suffered from inflammation of the lungs, and since then had not been strong. Though of phthisical appearance he had no distinct symptoms of phthisis. As a hammerman he found his occupation very trying, and often complained of a severe pain about two inches below and to the inner side of the right nipple. This pain was strictly circumscribed in character, covering an area of about a five-shilling piece. It was not constant, but he says was always very acute when he caught cold. In the morning of the day I saw him, he went out to his work in his usual good health and spirits. It was a very windy morning, and he had a mile and a half to walk. While on his way, but not specially exerting himself, he was seized with a sudden and acute pain in the area just described, which gradually grew worse, and his breathing became more difficult. In half an hour the dyspnœa was so great that he had to be brought home in a cab, and within an hour afterwards I saw him in the condition above described.

When I visited him the following morning, I found the pleural cavity still contained a considerable quantity of air, the percussion note tympanitic, though not of such a high pitch as before. He was lying on his back breathing naturally. Around the seat of puncture and extending in all directions there was considerable emphysema of the cellular tissue, the crepitation being such, on application of the stethoscope, as to mask the characters of the respiratory sounds. The pulse and temperature were quite normal. He had no cough or pain in breathing; he complained, however, of severe pain, commencing at the old spot, and extending up the right side of the sternum, aggravated on any movement, and relieved while he lay upon his back.

The two following days, the 12th and 13th, he continued to improve, the pain becoming less, the percussion note not so tympanitic, and the cellular emphysema less extensive.

As he could not be properly attended to at his own home, I had him removed, by the kind permission of Dr Grainger Stewart, to Ward 7 Royal Infirmary, where his progress could be carefully watched.

The following extract I take from the report of his case in the ward journal, which indicates his thoracic condition eight days after the commencement of his illness:—

"Upon percussion there is some hyper-resonance over right lung anteriorly; posteriorly and laterally, the percussion note is the same on both sides. Upon auscultation on right side, anteriorly, sounds are feeble and distant; vocal resonance is diminished. Posteriorly, sounds very distinct, vocal resonance normal. On left side, vocal resonance is slightly increased. By measurement, right side of chest is 17½ inches, left side 16 inches, making an increase of 1½ inches on the affected side."

From day to day his condition improved, and as the air in the right pleural cavity became absorbed, the lung expanded, and normal respiration was established.

This, as regards its onset and physical signs, was a typical case of pneumo-thorax. The diagnostic symptoms were so distinct as to exclude the possibility of error.

What then was the cause of pneumo-thorax in this case, and how did such a quantity of air get into the pleural cavity? Air may be introduced into the pleural sac,—

I. By conditions in the lung causing penetration of the pleura from within, or degenerative changes on the surface of the pleura causing penetration of the lung substance from without; in either case, opening up a communication between the lung and pleural cavity.

II. By an opening through the wall of the chest, and direct communication with the external air.

III. Possibly by the secretion of air by the pleuræ, or air generated by the decomposition of fluids already in the cavity of the pleura.

IV. By perforation of the pleural sac by degeneration and ulceration of growths in the œsophagus, stomach, or colon.

My case came under the first class of conditions. An opening, probably of small size, had been formed through the pulmonary pleura. With each respiration, air had been drawn into the pleural sac and a portion of it retained there, the amount of air increasing with the number of respirations, till the lung got quite compressed against the spine, and the wall of the chest became enormously distended, seriously displacing neighbouring organs, and giving rise to very alarming dyspnœa.

What, therefore, was the condition of lung which gave rise to the accident? Did it arise from gangrene of the lung, bursting of a superficial pulmonary abscess, breaking down of a tubercular mass, and ulceration taking place through the pleura, or rupture of an emphysematous portion of lung? Neither the history of the case nor the symptoms indicated either the existence of gangrene of the lung, or pulmonary abscess. A communication with the interior of the lung must, therefore, either have taken place through a tubercular cavity, or through the giving way of an aggregated mass of emphysematous air-cells.

I examined his chest carefully during his convalescence, and could detect no evidence of the existence of a cavity even of small size. No pleurisy ever was set up, no metallic tinkling could be heard, and no expectoration followed the occurrence; all of which phenomena were naturally to be expected had a communication been effected between a tubercular cavity and the pleura.

On the other hand, the acute pain at the onset of the attack took place at a part where aggregated emphysematous air-cells would likely exist, and where a tubercular cavity very rarely is found, namely, at the anterior margin of the lung, inferior and internal to the nipple. The patient was by occupation a hammerman, which would, no doubt, favour an emphysematous condition of the lung, and that such a condition had long existed, his barrel-shaped chest amply proved. While walking to his work the emphysematous part probably became over-distended, and gave way.

This, I think, may be the explanation of this case. The interesting points about it are, the absence of pleurisy, the rapid absorption of the air, and the non-recurrence of the accident.

The absence of pleurisy may be accounted for in two ways—(1) The aseptic character of the air which entered the pleura; and (2) Its speedy artificial liberation. We know that when air has passed through the minute air-tubes of the lung, it is rendered aseptic by being deprived of its septic elements, and hence probably its bland and unirritating effect upon the pleural membranes. The air in this case had travelled through the smaller tubes to the emphysematous region where rupture took place, and therefore was unattended by direct irritation. Had the air entered from a phthisical cavity, it would have passed directly from a larger air-tube into the pleural cavity before being rendered quite aseptic, and would most likely have caused irritation and pleurisy, combined, as it would have been, with particles of tubercular matter and blood.

If it be the case that air, whether aseptic or not, may excite pleurisy simply by mechanical irritation, this cause was removed by all tension and pressure being at once diminished after the introduction of the trochar and canula. By this operative procedure, the greatest portion of the air was got quit of. Of what remained, a considerable part made its way into the cellular tissue, giving rise to emphysema, and in four days was absorbed, while the remainder must have been absorbed by the pleural serous surfaces themselves, and with wonderful rapidity, viz., in about three weeks after the occurrence of the rupture.

Whether there will be a recurrence of the pneumo-thorax it is impossible to say. Up to the present time there has not been the slightest appearance of it, and it is possible that nature may have accomplished such changes as to render this accident unlikely. The opening through the pulmonary pleura was no doubt small and valve-like, so that air could pass into the pleural sac, but none could pass out. The pressure from the compressed air must have been very great—great enough, perhaps, to consolidate what was formerly emphysematous lung. At the same time, a small blood-clot would probably form, which would become organized during the quiescent state of the disabled lung, and effectually plug up the opening. Or if a blood-clot were absent, a small quantity of lymph would be thrown out, and agglutinate the raw edges of the wound.

As regards the operation, nothing could be more simple, and at the same time safe and effectual. Safe, however, only in so far as it was done antiseptically. By placing a fine piece of linen, dipped in carbolic-acid solution, over the canula, a constant antiseptic medium was provided, which rendered the spray only necessary at intervals. Besides this, by simply noticing whether the guard over the mouth of the canula was drawn to or repelled from it, an indication was given as to whether air was passing into or expelled from the chest. Thus, by a little care, the finger could be placed over the canula on any indication of air being drawn in, and immediately raised when air was being forced out; or, in other words, the finger acted as a valve, which allowed only of the passage of air outwards. Had this not been the case, the lung, pressed upon by air from within and without, would have remained in a state of collapse from its natural elasticity, and consequently much less air would have been expelled from the cavity.

By the use of the aspirator, all danger is avoided, and I think it should be used in preference to the method I have just described; but where this instrument, or even the spray, cannot be had, a piece of linen, saturated in some antiseptic, and placed tightly over the canula, will prove a very efficient substitute in an antiseptic point of view.

ARTICLE VI.—*On the Practice of Medical Jurisprudence.* By H. D. LITTLEJOHN, M.D., F.R.C.S. Ed., Surgeon of Police and Medical Officer of Health for Edinburgh; Lecturer on Medical Jurisprudence and Hygiene, School of Medicine, Edinburgh.

NO. IV.—POST-MORTEM EXAMINATION.

B. *Internal Examination.*

A MEDICO-LEGAL dissection differs widely from one instituted for purely pathological purposes. In both, the cause of death is sought to be ascertained; but, that once determined, the pathologist cares little for the precise *mode* in which the fatal issue was brought about. Whether a thoracic tumour occasioned death by pressure on the windpipe, thoracic duct, or on the heart itself, is a question of merely secondary importance to the pathologist, compared with the determination of the exact nature of the morbid mass, its site, and its progress in the invasion of various organs. Hence it is that, in ordinary dissections, points which are of great value to the medical jurist are overlooked, and others, which cannot be omitted in a medico-legal report, scarcely attract attention. The exact relations of the rigidity and lividity, for example, the state of the windpipe, the fulness or otherwise of the two sides of the heart, the condition of the blood throughout the body, and the various internal changes due to the advance of the putrefactive process, simulating, as they do, disease and the action of poisons, as well as their effect in exaggerating injuries and diseased appearances, are not carefully estimated by the pathologist.

In a dissection ordered *by the authorities*, an important question meets us *in limine*, which is of little or no account to the pathologist, and must be answered—viz., "What organ of the body should be first examined?" Having made our inspection of the external surface, shall we commence with the head and proceed downwards, examining in rotation the organs of the chest and abdomen? Is it necessary, in all circumstances, to examine the windpipe and air-passages, and also the state of the spine, more especially of the cervical portion? These and similar questions, which emerge in the course of an official post-mortem examination, we propose to answer, as we give a description of the progress of a medico-legal dissection such as will be easily followed by the practitioner of medicine, for whose use it is intended, and which will at the same time satisfy the requirements of criminal legislation.

It is a trite observation, that whatever is worth doing is worth doing well; and although neatness and cleanliness should characterize every post-mortem examination, these qualities are specially necessary in one ordered officially. The magistrate or the public prosecutor may remain to witness it, and as their experience of such dissections may be large, they are excellent critics, and form judgments which may, or may not, tell to the advantage of the medical inspector.

Hence, even in such a simple matter as exposing the sternum and ribs, a neater dissection will be made by dividing the attachments of the pectoral muscle, and then, by steady traction and a few sweeps of the knife in the cellular tissue, laying bare the ribs and intercostal muscles, than by leaving longitudinal ridges of muscular tissue adhering to and obscuring the ribs and cartilages.

The 2d, 3d, 4th, 5th, and 6th superior costal cartilages should be divided on either side, close to their junction with the rib, and those of the 7th, 8th, 9th, and 10th transfixed from below upwards, and divided from within outwards, so as to avoid wounding the liver and stomach. By simply bending the abdominal parietes upwards and backwards over the lower portion of the thoracic wall, its free edge is exposed, and a single incision suffices to dissect off the structures covering it. The attachment of the diaphragm to the sternum and cartilage is now to be divided, and the right hand passed upwards into the anterior mediastinum, breaking up the cellular tissue, and exposing to the touch the organs as high as the root of the neck. The advantage of this procedure is, that it frequently enables the operator at once to discover the probable cause of death. Any tumour, whether aneurismal or otherwise, is thus readily detected, and some notion is gained as to the size of the heart and the condition of the lungs. Nothing can be more embarrassing than to mutilate an aneurismal sac in removing the sternum; whereas the existence of a swelling in the neighbourhood of the aorta having been ascertained, greater care is exercised in separating the sternum, and the diseased structures are exposed in an uninjured condition. The sternum is removed in one of two methods—either entirely by disarticulation, or partially by sawing through the bone immediately below the attachment of the cartilage of the first rib, and thus leaving the manubrium with its clavicular articulations. We are clearly of opinion that the latter is the plan that should always be adopted. In removing the entire bone, we run great risk of wounding the large veins at the root of the neck; and as these vessels are, in a large number of cases, distended with *fluid* blood, the dissection is at once obscured by an immediate flow which cannot be stanched, and which undoubtedly interferes with an accurate determination of the amount of blood in the right side of the heart. But, in addition, so soon as the clavicular attachments of the sternum are divided, the weight of the shoulders drags the clavicles outwards, and leaves a deep sulcus on the sternum being removed. When the dissection is completed and the incision is sewn up, an awkward hollow remains, producing great disfigurement, and suggesting at once to the relatives the removal of internal organs. All these annoyances are avoided by leaving the upper part of the sternum in the manner indicated.

The thoracic and superficial abdominal organs being now exposed, the question, what organ should be first submitted to examination, must now be determined. Authors generally lay it down as a rule that the brain should be examined in the first place, and that the dissection should be carried on from above downwards. In a medico-legal dissection, we are clearly of opinion that the heart should be the first organ examined, and that this examination should take place *in situ.*

Before, however, this organ is touched, the medical examiner may be able, from the appearance of the pericardium, to hazard a confident opinion as to the cause of death. A somewhat distended condition and a bluish coloration of this sac indicate either rupture of the heart or of the aorta, and the presence of blood in the pericardial cavity. Where no such appearances present themselves, a simple incision serves to expose the surface of the heart, and the condition of the organ is at once ascertained. The state of the cavities of the right side, and of the pulmonary artery, whether distended or otherwise, is easily made out, and simple palpation indicates whether the blood is fluid or coagulated, or whether a mixed condition is present. By now raising the organ, the condition of the left auricle and ventricle and of the cardiac veins is seen, and also the amount of serum in the pericardium.

Having ascertained these important facts, which cannot be determined with accuracy, as we shall see, at a later period of the dissection, we proceed to the examination of the head.

Head.—The condition of the scalp first engages our attention, and an incision is made from ear to ear. In those who have had this part of the body repeatedly subjected to violence by bruising and laceration, the scalp is unusually adherent to the pericranium, and some notion, therefore, of the habits of the deceased may thus be gained by the facility with which the scalp separates. Where injuries are discovered, it is of importance to note, in addition to their general appearance, the amount of surrounding ecchymosis and its exact position, whether strictly limited to the scalp, or lying upon or beneath the pericranium.

No instrument has ever been devised which effects the separation of the skullcap better than the ordinary saw, but to ensure dexterity in this operation, certain precautions must be attended to. A circular incision must be practised round the skull, immediately above the superciliary ridge in front and above the occipital protuberance behind. This incision must divide the pericranium and indent the bone. Where it crosses the temporal muscle its route must be exposed by carefully separating the muscle. A well-defined route is thus made for the saw, which ought to be of good size, with its teeth widely set, as the generality of these instruments are set too finely; their teeth thus becoming obliterated with fat, and requiring repeated cleaning during the dissection. The saw should be passed rapidly along this incision so as to form a well-marked sulcus. The remaining procedure is comparatively simple: the instrument can be urged with greater rapidity in the well-defined route, care being taken never to press heavily upon the saw, but to allow it, so to speak, to do its own work. Perfection in this operation would consist of such a division of the external table as to leave the internal table comparatively uninjured; but from the great inequality of the thickness of the skull this cannot be accomplished, and hence it happens that, in the most skilful hands, the escape of blood or serum testifies to the partial opening of the cranial cavity, and also too generally to the injury of the membranes and brain. The smaller the amount of this escape and injury the better; and the operator should carefully note the peculiar sensation communicated by the saw as it enters the diploe, and, when that is felt, should proceed no further.

No practice is to be more reprobated than the employment of a hammer or wooden mallet to crack the internal table and separate the skullcap. Should the cause of death prove to be injury to the brain and fracture of the skull, this violent procedure must always leave us in doubt as to the exact causation of a fissure in the cranial bones; and we are sure that were the counsel for the defence, in a case of murder by injury to the skull, to produce a mallet in open court, and ask the medical witness to describe to the jury the procedure adopted in examining the cranium and detecting the fissure that led to the fatal hæmorrhage, considerable doubt would not fail to be raised as to the exact cause of death and the responsibility of the prisoner. We hold it to be essential that as little violence as possible be applied to the cranium in the separation of the skullcap; and for the last twenty-five years have

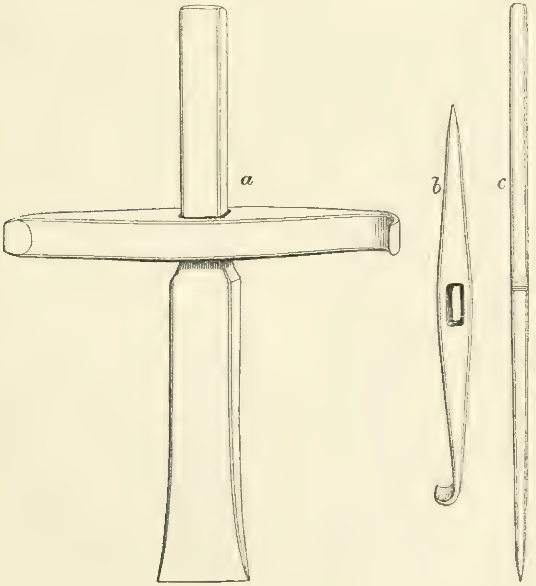

Fig. 1.

used with satisfaction and success the following adaptation of the ordinary chisel. See Fig. 1. A stout crosspiece of steel, *b*, is loosely fitted by an aperture to the chisel, and, gravitating by its weight, is arrested at the shoulder of the instrument, forming an ordinary cross, *a*, where the two portions are adjusted.

By sawing deeply over the frontal sinuses the sharp end of the chisel is easily inserted, as shown below; and by simply turning the chisel round, the internal table at once gives way with a slight crack, and the skullcap is felt to be detached. Indeed, the adhesions may be so complete, that after the skullcap has been divided, it still remains firmly attached, and can only be separated by the employment of considerable force.

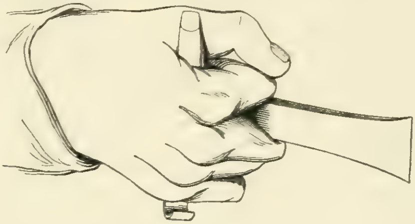

FIG. 2.

As the majority of cases of sudden or suspicious deaths investigated in large towns are of persons who have been addicted to the immoderate use of spirits, strong adhesions exist between the cranium and membranes, and it is no easy matter to separate them without injuring the fingers. It is well known that there is no more fruitful source of whitlow and blood-poisoning than such wounds, which, in the hurry of a dissection, are apt to be occasionally overlooked. By a simple addition to the crosspiece of the chisel, of a blunt hook, all risk of injury to the fingers is avoided. The hooked extremity of the chisel is passed under the front of the skullcap, as shown in Fig. 3, the position of the hand being

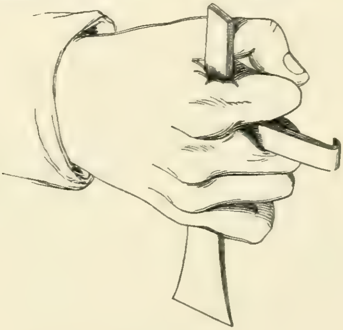

FIG. 3.

reversed, and the instrument so grasped as to enable the operator to employ steady and strong upward traction.

During this simple operation, important information, as we have seen, has been gained as to the habits of deceased, or as to the probable character of the diseased conditions within the cranium.

The dura mater is now carefully sponged, and should it and the other membranes have been accidentally injured, the escape of serum and blood is noted. We need hardly add, that such almost unavoidable accidents are never mentioned in the report of the dissection, although we have known of reports where these misadventures were carefully detailed. Such scrupulosity is of no use, so far as the ends of justice are concerned, and serves no other purpose than to encumber the report and embarrass the medical witness with awkward questions in the witness-box. In injuries of the head, it frequently happens that the simple removal of the skullcap discloses the nature of the case; a dark coagulum is seen lying on the surface of the membranes against the parietal and temporal bones, and flattening the side of the hemisphere. Whenever this is observed, the interior of the skullcap should be washed and examined for the extremity of a fracture, as it rarely happens that a fracture of the base of the cranium, and implicating the middle meningeal artery, does not reach upwards to the vault; and should a fissure be detected in the skullcap, the direction of the fracture becomes at once apparent. The membranes are now pinched up with ordinary forceps at either side of the falx, in the frontal region, and a small rounded portion removed, so as to admit of the entrance of a blunt-pointed bistoury, which, being passed outwards, divides the membranes from before backwards. We have found the scissors figured below very useful for the purpose. They are figured and described in Goubert's Manual de l'Art des Autopsies, Paris, 1867. The rounded blunt point is easily inserted, and can be used in addition as an ordinary probe.

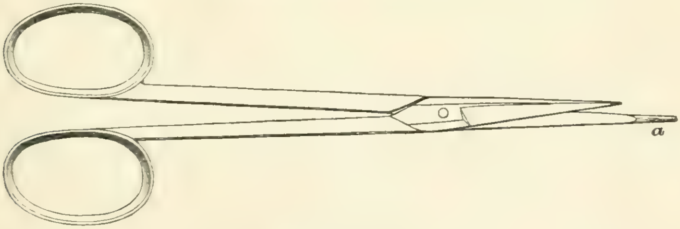

FIG. 4.

The surface of the brain having been exposed, the following points specially engage the attention of the inspector, viz.:—The state of the arachnoid as to opacity, the amount of the sub-arachnoid fluid, the degree of congestion of the veins, the consistence of the brain, and the general appearance of the convolutions. The summits of the hemispheres are to be removed with a flat knife, and the cut surface to be examined *without being subjected to any pressure.* Should numerous red points be at once visible on the pinkish surface, and blood escape on gentle pressure, the cerebral tissue is congested. Repeated slices are similarly removed until the level of the corpus callosum is nearly reached, where, should there be unusual accumulation of serum or other fluid in the lateral ventricles, these cavities will be observed to be distended and bulging upwards. In these circumstances it is easy to reach their interior, but when they are nearly empty and the brain firm, unless the incision be made close to the margin of gray matter surrounding the corpus callosum, the operator will fail to reach the ventricles, and he will find that his knife has entered the corpus striatum. This is embarrassing, and we have heard it hinted, in these circumstances, by a disconcerted operator, that the cavities in question were absent. The simple rule just indicated must be followed, and the slightest appearance of fluid carefully looked for. The fluid should be collected in a tube by means of a pipette, and reserved for future examination. The whole length of both ventricles is to be freely laid open. The remainder of the brain is to be detached from before backwards, dividing the various nerves and vessels, and cutting through the tentorium cerebelli. The cord is to be divided by two lateral incisions as low down as possible, and the various nerves and vessels at the side cut through. By now tilting the brain and cord upwards, the whole mass is removed. As this is effected, the amount and character of the blood and serum collected at the base and in the spinal canal are estimated. The base of the brain is turned upwards, and by two lateral cuts through the crura cerebri the cerebellum and spinal cord are detached. Grasping these parts with the left hand, so that the anterior surface of the medulla looks upward, one of the cerebellar lobes is perpendicularly sliced from its outer surface until the pons and medulla are reached. The cerebral vessels are examined for any spots of atheroma; very thin slices are made of the pons and medulla, each slice as it is cut being turned over by a slight jerk of the knife and gently scraped, so as to expose the surface of the incision as free from blood as possible. By this necessary procedure, the condition of this most important part of the nervous system can be accurately determined. The remaining lobe of the cerebellum is similarly examined. The remains of the two cerebral hemispheres are now separated longitudinally along the middle line. Each in turn is held in the left hand, the inner surface looking upwards. With the right thumb the choroid plexus is carried down to the transverse fissure, when, by gentle traction, the membranes are stripped from the under surface of the brain. The central ganglia, being now freely exposed, are carefully divided by successive horizontal sections. A few incisions into the anterior and posterior portions of the hemisphere complete the examination of the brain and appendages. One necessary duty must not be neglected, viz., to test, from time to time during the dissection, the presence of alcohol. It is often of great importance in a criminal case to be able to state whether any alcoholic odour was present in the brain, and the inspector, therefore, should search for this on removing the skullcap, on incising the substance of the organ, and especially on collecting the ventricular fluid, and when the base of the skull is exposed. The membranes at the base of the skull are examined as to opacity, etc., and then removed from the bone; great assistance in effecting this will be obtained by using, as a wedge, the opposite extremity of the crosspiece of the chisel, which is bevelled so as to serve as a screw-driver. See Fig. 1, *b*. It frequently happens, in hurriedly ordered examinations, that the coffin is screwed down before the arrival of the inspectors, and difficulty and delay are experienced in getting a screw-driver. The annoyance is obviated by this simple arrangement.

The *throat* and *windpipe* are now examined by making an incision from the under surface of the chin to that already practised on the chest and abdomen. Should there be no appearance of injury to the skin such as abrasions and scratches, or to the parts below as indicated by ecchymosis, the incision is to be carried down to the windpipe and along the under surface of the tongue. The knife is then to be passed along the inside of the lower jaw, dividing the mucous membrane of the mouth, until the finger can be freely passed along the upper surface of the tongue. Any obstruction or foreign body is thus easily detected. The knife is now passed freely down on either side of the tongue and windpipe, and the finger continuing to press the tongue downwards, the entrance of the pharynx is exposed. A sweep of the knife transversely through the velum palati brings the back wall of the pharynx into view, which, in its turn, is cut across, and a few touches of the knife serve to detach the gullet and windpipe, bringing their superior orifices into full view. This must be done rapidly, for should the veins be distended with fluid blood, there is every chance that the blood will escape freely and enter the larynx, not only obscuring the dissection, but also throwing doubts on the condition of that important cavity. Any foreign bodies and particles are to be specially looked for, and the gullet laid open with the scissors already described, Fig. 4. It is rare that anything abnormal is observed in the gullet; but the epiglottis and upper strait of the windpipe must be specially examined for any foreign substances. The back of the larynx and trachea is to be freely incised, and the state of congestion of that portion of the windpipe, and the presence of frothy mucus or of foreign particles carefully noted. The parts in question are allowed to fall forwards, exposing the front of the cervical vertebræ. This portion of the spine can now be satisfactorily examined. By introducing the forefinger of the left hand into the foramen magnum, and grasping the front of the vertebræ with the right hand, the condition of this important part of the spine can be accurately determined. When treating of reports of cases of asphyxia and injuries to the spine, we shall show how essential it is in all cases of sudden and suspicious death, not only that the windpipe should be examined, but also that we should be assured of the integrity of the cervical portion of the spine.

Chest.—The organs of this cavity are now subjected to more searching examination. As the removal of the heart necessitates the escape of blood, it is advisable, before the dissection is thus obscured, to pass the hand into the pleural cavities, to determine their contents and the presence of adhesions, etc. The surface of the lungs should also be inspected. An incision is now made into the right ventricle and along the course of the pulmonary artery, to ascertain the presence and character of clots, or whether the blood is fluid. Should clots be found, they are to be removed—their terminations and consistence observed. A decolorized clot in the right ventricle, extending into the pulmonary artery and its branches, negatives the supposition that asphyxia was the cause of death, and gives important information as to the rapidity of the death. The organ is next to be removed by cutting through the aorta (about an inch above where it first makes its appearance) and the other soft structures, until the inferior vena cava is reached and divided. The heart is now to be held with the apex downwards, and put aside for more minute examination. Nothing can be more slovenly than, as is too often practised, to drag the separated heart, base downwards, across the face of the deceased, dripping it with blood. Such carelessness is at all times to be reprobated, but more especially in a medico-legal dissection, where legal functionaries may be present. The state of the valves and of the cardiac substance is to be specially noted. For this purpose the under surface of the organ being turned upwards, the forefinger of the left hand is to be passed down the aorta into the ventricle, when possibly disease of that vessel and of its valves will be at once detected. The finger is kept close to the septum, and the thumb of the same hand pressed against it from the outside. Immediately in front of the tips of these fingers, the left ventricle is to be transfixed with a knife, which is carried rapidly forwards to the apex, thus laying the cavity of the ventricle open. The heart is now suspended by the aorta, each side of the cut edge of the vessel being held asunder, so as to permit the competency of the valves being tested by pouring water down the vessel. The valves themselves are now to be exposed, which is effected by the heart being held base downwards by the outer wall of the left ventricle, while an assistant, at the same time, seizes the apex of the right ventricle and holds it apart. The knife is carried downwards along the former incision until the aorta is reached, when it is easy to pass the knife between any two of the valves, and thus complete the division of the vessel. The mitral valve is now examined, and two fingers passed through it to test its patency. The valves on the right side and the various sections of the cardiac tissue are looked at, and, finally, a small portion is removed from the septum for microscopic examination.

We have already alluded to effusions of blood into the pericardium. The source of the hæmorrhage must be either the heart or great vessels; and in order to ascertain the exact site of the rupture, the heart must on no account be separated from its attachments. Cases have been recorded of death from pericardial effusions of blood, and where the dissection has been made by persons of skill, who failed to detect the origin of the hæmorrhage. The failure is easily explained. A minute aneurism at the commencement of the aorta has given way—the rupture, possibly, being no larger than a pin's head. In cutting out the heart, the incision of the aorta has involved the ruptured orifice, thus preventing its being detected. Now, the rule to be attended to in these circumstances is, never to remove the heart until the site of the rupture is found; and the following is the manner in which the dissection must be conducted in order to insure this being discovered: The heart having been examined for any rupture of its substance—the anterior surface and that portion lying close to the septum being the usual site—the left ventricle is to be incised, and the forefinger of the right hand passed up into the aorta. As the ruptured orifice is generally situated anteriorly, by simply pressing the finger against the wall of the vessel, the slightest depression is detected, and there, on closer examination, the minute rupture is discovered. Should any doubt still remain, the aorta may be compressed at the arch, and some water injected upwards from the ventricle, when a tiny jet will quickly reveal the site of the aperture.

Each lung ought next to be removed and incised, and the condition of these organs as to congestion, etc., determined. With the scissors already mentioned, the pulmonary vessels and the bronchi are slit open, and the presence of clots and the condition of the mucous membrane as to mucus and congestion specially noted. The walls of the chest are now to be examined after the removal of the heart and lungs, and of the blood and serum that may have escaped. Any fracture or other injury can thus be ascertained, and by pressing the chest from above downwards, while the right hand is passed over the pleural surface, no fracture of the ribs can possibly escape detection.

In all cases of death there is more or less congestion of the posterior and depending portions of the lungs, and occasionally a considerable amount of solidity is in consequence communicated to the touch in handling these organs. But all doubt as to the character of this pseudo-condensation is removed by observing its position, the absence of frothy serum, and also that, on incision, the slightest pressure produces on the cut surfaces numerous spots of fluid blood closely resembling black currants. The simple experiment of testing the specific gravity of a portion of the lung by placing it in water should in all cases of uncertainty be practised.

Abdomen.—In cases of sudden death where a satisfactory cause has been found—say, in the chest—the examination of the organs of the abdomen can be rapidly effected. The upper and under surface of the liver is inspected, its substance incised, and the gall-bladder laid open. The spleen is looked at and incised. Both kidneys are removed, laid open, and their investing membrane stripped off. Lastly, the stomach is laid open *in situ*, its mucous coat and contents examined and smelt. The small and large intestines are here and there cut into to ascertain their condition, and the presence or absence of food and fæces. The state of the bladder is also noticed.

Should, however, the previous part of the dissection have furnished no sufficient evidence of the cause of death, then the abdominal organs must be examined with great care. The first duty of the inspector is undoubtedly to apply a double ligature to each end of the stomach, and to remove this organ and its contents for separate examination. Meanwhile, the liver is to be detached by cutting the broad and the lateral ligaments, exposing the lobus Spigelii, and, having passed the left forefinger under the mass of vessels and the duct, dividing them. The vena cava and the attachment to the diaphragm are now the only obstacles, and when these are separated, the liver can be removed and weighed. It should then be deeply incised, so that its structure and the amount of blood in it may be determined. A small portion should be retained for examination under the microscope. The contents of the gall-bladder should be looked at. The spleen is detached, weighed, and examined by incision. Both kidneys are to be exposed, removed, and incised. The amount of blood in them is determined by squeezing the cut surfaces, and the capsules are then detached, due observation being made as to whether they separate easily or only imperfectly. The surface of the organ can now be carefully examined, and a portion reserved for the microscope.

The ligatures at either extremity of the stomach are then removed, and the organ laid open, care being taken to receive the contents in a clean vessel. These are first carefully smelt, and the stomach spread over the extended left hand, so as to permit of a minute inspection of the mucous coat. By this procedure, any foreign particles can be detected, as well as any ulceration; and should marks of congestion be present, their distribution and character can be determined. The intestines are now separated, and their contents set apart for special examination. The scissors previously figured, p. 1117, will be found useful in the examination of the intestinal canal. The bladder is opened, and should urine be present, a specimen should be preserved. Lastly, the hand is passed along the spinal column to assure ourselves of its integrity.

In the event of blood being found effused in the abdominal cavity, or of peritonitis being present, a similar caution to that which was given when discussing the origin of hæmorrhage into the pericardium must also here be insisted on. On no account must any incision be practised, or parts removed from the cavity, until the site of the rupture has been ascertained. Should the body have been subjected to violence, the blood, in all probability, has escaped from a rupture of the liver or of the spleen. This can be satisfactorily ascertained by sponging out the blood, and examining these organs in detail. The rarer occurrence of aneurism or wounds of vessels from pelvic fracture occasions no difficulty. In peritonitis which is suspected to be the result of violence, the folds of intestine must be carefully separated, any fluid effusion removed by sponging, and, commencing at the cæcum, the intestine must be examined inch by inch until the rupture, which may be of small size, is detected. Should such be found, its exact position may be indicated by transfixing the adjacent mesentery with a pin. It is necessary that the whole length of the intestinal canal should be inspected, to obviate the objection which might be urged, that the small rounded opening described was an ulcer which had accidentally given way. Should we omit to examine any portion of the intestine—say, the duodenum—we are not in a position to state positively that the intestinal canal was not ulcerated. And while we may be perfectly satisfied that the aperture spoken of was a rupture occasioned by violence in a healthy intestine, by the neglect of what we consider a necessary precaution, we expose ourselves to the question, "Might there not be in that portion of the intestine which you did not examine, ulcers similar in size, but which were superficial, and had not perforated the bowel?"

Again, in injuries inflicted on the internal organs of generation in the female, it is of consequence, in order to make a satisfactory dissection, that, after removal of the intestines, the pelvic contents be first carefully examined from above, and that on no account should they be removed until the presence and relations of any wound or effusions of blood be clearly made out. When this examination *in situ* is made, and not till then, the pelvic organs are removed by running the knife along the brim of the pelvis, separating the cellular attachments, and dividing the rectum and vagina. By following this rule, much uncertainty is avoided in the subsequent direction of injuries. Trials for criminal abortion have occurred, where, from the careless manner in which the dissection had been made, the medical men who appeared for the defence have been unable, from the medical report adduced for the prosecution, to follow the direction of the so-called wounds, or even to understand the precise nature of the injuries.

The organs having now been removed from the body, the dissection is completed by examining the back of the trunk and limbs for any injury or unusual appearances.

In this description of an ordinary medico-legal dissection, we have specially insisted—

1*st*, That the external examination should, in the first instance, be limited to the front of the body.

2*d*, That the organs of the chest and abdomen should be first exposed.

3*d*, That the heart should be carefully examined *in situ*.

4*th*, That the inspection should then be proceeded with from above downwards.

5*th*, That no hammer or mallet should be used in detaching the skullcap.

6*th*, That to enable us to form an estimate of the condition of deceased as to sobriety at or about the time of death, and also as to his general habits, any serum in the ventricles of the brain should be preserved for the detection of alcohol, and portions of the heart, liver, and kidneys submitted to microscopic examination.

7*th*, In ordinary circumstances, where the dissection satisfactorily disposes of the case, it is not necessary to take the trouble of weighing the various organs where these present no unusual dimensions or appearance; and such phrases as "enlarged in size" or "much enlarged," etc., may be employed; but, in suspicious cases, remembering that our report must convey as much information as possible to those whose advice may be asked to support the allegations of a prisoner, the weight of the principal viscera, more especially of the heart, liver, spleen, and kidneys, must be given, should these not be described as normal; care being taken at the same time to indicate the size and configuration of the body of deceased. Before the introduction of spring balances, which can now be had of small size and moderate in price, it was a valid excuse for the absence of the weight of viscera in a report, that the dissection was conducted in the country under disadvantageous circumstances, a scale and weights not being obtainable. Now, however, this plea can no longer be urged.

8*th*, When, after a searching examination, we have failed to ascertain the cause of death, our duty is plain. We seal up and retain the contents of the stomach and intestines, and we examine with the microscope the portions which we have removed from the heart, liver, and kidneys. These organs may prove to be so diseased as to enable us to advise the authorities that an analysis of the contents of the digestive canal is not necessary; but should the microscope give no positive indication of the presence of disease, then our report must be a negative one; and we place in the hands of the authorities the vessels, duly authenticated, in which we have secured the contents of the stomach and intestines.

Part Second.

REVIEWS.

Clinical Lectures on Diseases of the Heart and Aorta. By GEORGE WILLIAM BALFOUR, M.D. St And., F.R.C.P. Ed. London: J. and A. Churchill: 1876.

DR BALFOUR needs no introduction from us; he has been long known to our readers as the author of various important papers published in this Journal, and we have much pleasure in welcoming the present volume, containing his principal contributions to cardiac pathology. Most, but not all, of these lectures have already appeared in our pages, but all have been more or less altered, and to several important additions have been made.

We must at once express our regret that Dr Balfour should have decided to retain in this publication the form of lectures, and though we quite appreciate his desire to give as much as possible the results of his own observations, we must express the hope that, at no distant date, we shall see this volume expanded into a complete treatise on diseases of the heart. A great part of the work is already done, and the recasting of the present lectures, with the addition of a few chapters, is all that would be required. It is an important feature in Dr Balfour's writings, that he always endeavours to base his explanations of the phenomena observed upon physical laws, and the results of experimental physiology; and although our knowledge is in many respects deficient, this volume indicates what important results may be anticipated both in reference to the pathology and to the treatment of the diseases of the circulatory organs. Without further preface we proceed to make some remarks on a few only of the subjects treated of in the "Clinical Lectures."

The first chapter treats of the diagnosis of cardiac disease generally, with reference to the symptoms and the physical signs. This subject is considered under the heads of breathlessness, dropsy, examination of the pulse, inspection, palpation, percussion, and auscultation. The lecture forms an excellent introduction to the course, containing under the above heads a great deal of valuable information. There is only one statement we would object to, as being calculated to mislead the student. In reference to auscultation of the heart, Dr Balfour says, "Of all the signs of cardiac disease, murmurs are those most usually confided in, and yet they are really those of least value; first, because, as already remarked, exocardiac murmurs may simulate those of endocardiac origin; and, second, because murmurs, truly of valvular origin, may disappear temporarily or permanently," p. 34. Now, granting the truth of the qualifications, the main statement is in our opinion made much too broadly. In the very next lecture we are informed, "that amongst all the various signs which enable us to detect aortic incompetence or predict its significance, the murmurs which accompany it, and their mode of propagation, are not the least important," and that "a diastolic murmur at or beneath the level of the aortic valves in the line of the sternum, is pathognomonic of aortic regurgitation," p. 67; and again, "There is a murmur which is so invariably associated with disease of the mitral valve that, when once heard, it may be conclusively accepted as a distinctive proof of the existence of a permanent deformity, even though the murmur itself should subsequently disappear, as it frequently does," p. 102.

The second and third lectures treat of incompetence of the aortic valves. The chief object of the first is to show the different characters of the murmurs which may accompany disease of the valves, and to indicate how we are to determine the relative amount of obstruction or incompetency, the latter of course being much the more serious in a prognostic point of view. Four important cases are recorded, and the following are the conclusions which Dr Balfour draws from them:—"A diastolic murmur at or below the level of the aortic valves, chiefly audible in the line of the sternum, is significant of aortic incompetence. If this diastolic murmur be inaudible in the carotid arteries, it is invariably accompanied by a systolic murmur, having its maximum intensity at the aortic valves or in the so-called aortic area; and this indicates comparatively trifling incompetence with considerable obstruction at the aortic orifice, most probably from calcified semilunar valves. If this diastolic murmur be distinctly audible in the carotid arteries, it is invariably preceded by a loud systolic murmur in them, the systolic portion of the murmur being not always very audible in the aortic, or in any part of the cardiac area; and this indicates very considerable incompetence with comparatively trifling obstruction," pp. 67, 68. Now, although we agree to a certain extent with these opinions, we consider that Dr Balfour is too sweeping and dogmatic in his conclusions, particularly as in only one of the cases on which the conclusions are founded, was the evidence of post-mortem examination obtained. Indeed, in commenting upon one of these very cases, Dr Balfour says, "We are scarcely yet in a position to state in every case why a diastolic murmur originating at the orifice of the aorta should at one time be heard loudest above that part, as in the so-called aortic area, and at another lower down the sternum. At present we can only state it as a fact," p. 50. Our knowledge of the mechanism of cardiac murmurs is still far from complete, and we shall only expose ourselves to disappointment, by making claim to an accuracy which we believe is at present unattainable.

The third lecture is a very interesting one, treating of the results of aortic disease; of the mechanism by which in many cases compensation is provided; of the mode of death; and of the treatment of the affection. On the last-named point Dr Balfour's opinion as to the use of digitalis in aortic incompetence differs from the views commonly entertained; we must therefore defer our remarks on this point till we allude to his chapter on the therapeutics of cardiac disease.

The subject of mitral obstruction is one which Dr Balfour has made peculiarly his own; and the fourth and fifth lectures contain an exceedingly good account of the lesion. The murmur characteristic of mitral stenosis, presystolic, or rather auriculo-systolic in rhythm, is short and rough, precedes the carotid pulse and apex beat, and is often accompanied by a sensation of vibration or purring tremor over the mitral area. The murmur is still frequently described as diastolic, but there can be no doubt that it is produced by the contraction of the left auricle, which, as all physiologists allow, immediately precedes the contraction of the ventricle. It is, however, perfectly true that, though the characteristic murmur must be auriculo-systolic in rhythm, it may be accompanied by, or there may exist independent of it, a diastolic ventricular murmur. This fact is pointed out by Dr Balfour, who, in connexion with two very interesting cases, describes very clearly the mechanism of this diastolic murmur. In a normal condition of the mitral valve the diastole is completed at once, the blood passing freely from the auricle into the ventricle. Blood still continues to flow from the pulmonary veins into the left auricle, until the auricle, stimulated by distention, contracts upon its contents, which it forces into the already full ventricle, which in its turn is stimulated to contraction. In a healthy condition of the mitral orifice and valve, all this takes place without the production of sound. If, however, there is contraction of the mitral orifice, the ventricular diastole is necessarily prolonged, and it is a remarkable fact that this prolonged diastole, though not in general attended by sound, may be accompanied by a soft, blowing, or even by a musical murmur, which presents none of the rough character of the auriculo-systolic murmur. We see no other explanation of this diastolic murmur, though it is no doubt difficult to understand how the mere passive passage of blood through a contracted orifice should give rise to a murmur, especially as we know that a presystolic murmur caused by the active contraction of the auricle is frequently wanting. The changes which are liable to occur in the latter murmur are at present inexplicable; under apparently similar circumstances it may be constantly present, be persistently absent, or may appear and disappear in a quite unaccountable manner. But even when the pathognomonic murmur of mitral contraction is absent, other phenomena exist, which should leave no doubt as to the nature of the lesion. These are, as pointed out by Dr Balfour, a slightly thumping character of an impure first sound; irregularity of the pulse greater than is met with in any other form of cardiac disease; reduplication of the second sound; and a weakened character of the aortic, and an accentuated character of the pulmonary second sound. There is only one point, in reference to Dr Balfour's account of mitral disease, to which we must take exception, and it is this: At page 154, the author says, "Constriction of that (the mitral) opening is, except in the rarest cases, unfailingly present when the mitral valves are diseased." Had Dr Balfour limited his statement to cases of chronic and long-standing disease of the mitral valve, we should not have objected to it; but we have met with a certain number of cases of incompetence of the mitral valve, the result of ulceration of the flaps, or of breach of continuity of the chordæ tendinæ, in which no trace of constriction existed.

The sixth lecture is a very important one, and is devoted to the consideration of cases of curable mitral regurgitation. The fact that a murmur, indicative of mitral regurgitation, might be present independent of organic disease of the valve, has been long known, and has been generally ascribed to irregular contraction of some part of the cardiac muscle, particularly of one or other of the papillary muscles. This explanation of the murmur is, however, rendered improbable by the circumstance, that the murmur, instead of being intermittent, as it should be if of spasmodic origin, is unchanged while it lasts.

Dr Balfour accordingly takes up the position, that these murmurs are in reality due to dilatation of the left ventricle, and a resulting imperfection of the mitral valve. The sequence of phenomena appears to be this: In consequence of an impoverished condition of the blood, the nutrition of the cardiac muscles becomes defective, hence there is relaxation of the muscular tissue and dilatation of the ventricular cavity; the mitral orifice being also dilated, the valve is unable to close it perfectly, and hence a certain amount of regurgitation of blood takes place. Hæmic or functional murmurs are very frequently met with in chlorosis, and are heard in the veins in the form of a continuous hum; in the pulmonary, the aortic, and less frequently in the mitral area, in the form of a systolic murmur. Now, granting that the venous murmurs are due to anæmia or spanæmia, Dr Balfour's object is to prove that the cardiac murmurs are produced at the mitral valve in the manner already described. In doing so, he lays great stress upon the observation of Naunyn, that whether or not there be in the mitral area a systolic murmur, the bruit has its maximum intensity, not in the pulmonary area, but at least an inch and a half to the left of the left edge of the sternum in the second interspace—that is, just over the appendix of the left auricle—and that it is due to the murmur of mitral regurgitation being propagated upwards.

Now, while we perfectly agree with Dr Balfour's explanation of the murmur in the mitral area, and of the murmur over the auricular appendix, we do not agree with him in considering that "the arterial murmurs are therefore not of independent origin, but are propagated from the mitral valve, which permits slight regurgitation between its segments." We differ from him for two reasons: first, because, in our experience, the situation of maximum intensity of the murmur is by no means always over the auricular appendix, but may be in the pulmonary, or even in the aortic area; and, second, because mitral murmurs of organic origin are very rarely audible over the aortic cartilage. Although fully recognising the importance of "fluid veins" in explaining the production of murmurs, we believe that in the case of the arterial murmurs there is some abnormal relation between the blood and the walls of the blood-vessels, as the result of which sonorous vibrations are produced. But, be the cause of these murmurs what it may, Dr Balfour has done good service in insisting upon the fact, that mitral regurgitation is not necessarily incurable, but is often very amenable to treatment.

We regret that our space does not permit us to refer in detail to the next seven lectures, which treat of tricuspid regurgitation, of murmurs in the pulmonary area, of the variation and vanishing of cardiac murmurs, of irregularity of the pulse, of the secondary results of cardiac disease, of angina pectoris, and of inflammatory affections of the heart and its membranes. All of these lectures contain much valuable matter, and not only indicate a thorough acquaintance with the latest literature on cardiac pathology, but afford constant proofs that the principles laid down are not given merely on the authority of others, but have been tested by a large and intelligent experience.

The fourteenth lecture treats of the therapeutics of cardiac disease, the greater part of it, however, being devoted to a consideration of the action and uses of digitalis. We are glad to find that Dr Balfour has an intelligent confidence in the action of medicines, and does not sympathize with the scepticism which has been so prevalent of late years. As is well known, a great change has taken place in the views entertained regarding the action of digitalis. It was for long regarded as a sedative and depressant of the heart's action, but is now nearly universally recognised as a cardiac tonic and stimulant. Dr Balfour is, of course, a supporter of this latter doctrine, but, even among the supporters of this opinion, he is distinguished for the boldness with which he prescribes this drug. Many physicians, while prescribing digitalis in mitral disease, and in cases of simple dilatation of the heart, consider it not merely useless, but positively dangerous in aortic regurgitation. (See also Lecture III., pp. 97 *et seq.*) Dr Balfour holds a different opinion: "There is," he says, "no other disease in which this drug is of more value, and no other in which its curative action can be more sufficiently demonstrated than in this" (aortic regurgitation). The great danger, in aortic incompetence, is death from asystole, depending upon over-distension of the left ventricle. As soon as any regurgitation takes place, "the interior of the left ventricle is (when the patient is in the erect posture) being constantly dilated by a force equivalent to the weight of a column of blood the height of the cranium above the heart, and of the diameter of the ventricular lumen." The hydrostatic pressure accordingly increases with the gradual dilatation of the ventricle. Now, the value of digitalis consists in this, that by its judicious administration it can produce such an amount of tonic contraction of the ventricle as shall rather more than counterbalance the dilating power of the arterial column. In order that this effect may be produced, the drug requires to be given freely, because we desire to produce the commencement of its poisonous action, that is, a certain amount of contraction of the apex, and diminution of the cavity of the ventricle. This principle we look upon as thoroughly sound, and Dr Balfour's cases may be referred to as showing what excellent results may be obtained by the practice in question. There is, of course, a danger of pushing the drug too far, but so long as the flow of urine continues free, it is quite safe to continue its use. If, however, the quantity of urine begins to fall, and if the pulse commences to thump or to falter, the drug should at once be discontinued.

Among other adjuncts in the treatment of cardiac disease, Dr Balfour recommends arsenic in cases of angina, or as a cardiac tonic when digitalis would be inadmissible; morphia, nitrite of amyl, and chloroform, for soothing cardiac uneasiness; and iodide of potassium in syphilitic hypertrophy of the heart. Iron also is often a valuable adjunct to treatment.

The fifteenth lecture is headed, On mal-position of the aorta, due to rickets, simulating aneurism. To this chapter we would refer our readers, as treating of a subject which we are sure will be new to many of them. Two very interesting cases are recorded, in which, in connexion with sclerosis of the spinal column, and a certain degree of displacement of the heart, the symptoms of thoracic aneurism were very closely simulated.

The sixteenth lecture treats of the diagnosis of thoracic aneurism, while the seventeenth, and last, is devoted to the treatment of aneurism by iodide of potassium. Our readers are doubtless aware that Dr Balfour has devoted much attention to the treatment above indicated, and the last chapter of his book is very valuable as giving the results of an experience extending over upwards of eight years. The general result has been, "speedy relief to pain and suffering of every kind, while in a few there has been so complete a subsidence of the tumour, and relief to all the symptoms, as to amount to an apparently perfect cure." That these results are not exaggerated, our readers may satisfy themselves by referring to the details of the cases recorded. In order to produce the desired effect, the drug must be administered in full doses, and the only inconvenience attending it is, that unpleasant symptoms of iodism are sometimes induced. Dr Balfour recommends that thirty grains, three times a day, should be given from the first, as then a tolerance of the remedy is established more rapidly than if small doses be given to begin with. If symptoms of iodism show themselves, a full opiate, or a dose of chloral, may be given at bedtime, and, if necessary, the use of the drug may be intermitted for a day or two. The *modus operandi* of the iodide of potassium is somewhat obscure; some have supposed it to act by increasing the coagulability of the blood, but Dr Balfour is of opinion that it exerts some peculiar action on the fibrous tissue, whereby the walls of the sac are thickened and contracted.

In taking leave of Dr Balfour's lectures, we strongly recommend them to our readers, as giving the latest information on various points of cardiac pathology, as containing much suggestive matter, and as laying down sound principles for the treatment of cardiac disease.

RECENT TEXT-BOOKS OF PHYSIOLOGY.[1]

If the appearance of numerous text-books is to be regarded as an indication of awakened interest in any department of science, then there is probably no science which at present commands so much attention as physiology. On the table before us there are no fewer than eight volumes, each of which purports to give a systematic account of the present state of knowledge on physiological subjects, and each of which has made its appearance in its present form during the past twelve months. Manifestly these works have been written chiefly for two classes of readers—first, for medical students; and, second, for medical practitioners. No doubt the professed physiologist may also derive much assistance from their perusal, but it is presumed that he has already framed a system of physiology for himself, to which he is constantly adding new information from original memoirs, so that he does not require the guidance of any text-book written by another man. We shall therefore attempt to take a general survey of these books, without entering on minute critical details, which space denies, mainly with the view of expressing our opinion as to how far each is suitable for the medical man desirous of renovating and extending his knowledge; and for the student of medicine who, in the study of physiology, is acquiring information in what is fitly termed the "Institutes of Medicine."

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[1] 1. *Principles of Human Physiology.* By WILLIAM B. CARPENTER, M.D., etc. Edited by HENRY POWER, M.B., etc. Eighth Edition. London: Churchill: 1876. Pp. 1–1176.

2. *A Text-Book of Human Physiology.* By AUSTIN FLINT, jun., M.D., etc. London: H. K. Lewis: 1876. Pp. 1–976.

3. *A Treatise on Human Physiology.* By JOHN C. DALTON, M.D. Sixth Edition. London: Churchill: 1876. Pp. 1–825.

4. *Elements of Human Physiology.* By D. L. HERMANN, Professor of Physiology in the University of Zürich. Translated by ARTHUR GAMGEE, M.D., F.R.S., etc., etc. London: Smith, Elder, and Co.: 1875. Pp. 1–587.

5. *A Manual of Physiology.* By Professor KÜSS of Strasburg. Translated by ROBERT AMORY, M.D., of Maine. London: Baillière, Tindall, and Cox: 1875. Pp. 1–531.

6. *The Essentials of Physiology.* By M. W. HILLES. London: Henry Renshaw: 1876. Pp. 1–479.

7. *Nouveaux Eléments de Physiologie Humaine, etc.* Par H. BEAUNIS, Professeur de Physiologie à la Faculté de Médecine de Nancy. Paris: J. B. Baillière et Fils: 1876. Pp. 1–1140.

8. *La Biologie.* Par le Docteur CHARLES LETOURNEAU. Paris: C. Reinwald et Cie.: 1876. Pp. 1–554.

There are two kinds of text-books which have very different values. The one kind is a mere compilation, and indicates on the part of the author extensive reading, the power of seizing the gist of new statements and theories, and the faculty of lucid exposition. The author of such a work lays no claim to originality, except it be in the arrangement of the subject. He may not profess any views of his own, or give in the book any reflection of his own mind. The second variety of text-book is more, however, than a mere exposition. It is a view of the subject from the mental standpoint of a man who not only knows what others have written, but who has definite opinions of his own. These opinions may have reference both to the subject-matter and also to the mode of communicating information, and, of course, the latter are likely to be held by one who has had the practical experience of the lecture-room or of the laboratory, in addition to the mental labour of the study. Now, both kinds of books have their advantages and disadvantages. The mere compiler usually gives a more complete exposition and a fairer view of disputed points; but his work wants the stamp of originality and of personal interest. On the other hand, the author of a text-book who has made the subject the work of his life, is somewhat apt to overlook the work of others, to magnify his own, and to write in a dogmatic and self-satisfied style. His work, however, from its originality, and from the consciousness the reader has that he is perusing the thoughts of an individual man, is usually not only interesting, but makes a lasting impression on the mind. The compiler arranges, explains, and diffuses information; the original writer may found a school of thought. These statements are well illustrated by the books now under consideration. They may be appropriately divided into the two classes. The works of Carpenter, Hilles, and Letourneau, may be regarded chiefly as compilations; while those of Hermann, Küss, Beaunis, Flint, and Dalton, belong to the higher order of original works. To differentiate still further, it may be said that the most original of all these works are those of Hermann and Küss.

We shall now direct more detailed attention to the separate works. Dr Carpenter's "Principles" (No. 1) is a book so well known as to require only a brief notice. Under the able editorship of Mr Power, it must still be regarded, taking it as a whole, as the most complete exposition of physiological knowledge in the English language. The present edition, compared with the seventh edition, represents the advancement of physiology during the past five years. In its preparation, the editor has collated most carefully most of the recent contributions of physiologists, and he has got special and direct assistance from several. As a work of reference, it ought to have its place in the library of every medical man. There he will find a short and clear account of the most recent investigations, and no opportunity is missed of pointing out any practical influence the facts may have on our theories of disease. On the other hand, this is evidently not the book for the student of medicine. Not only is there no time, in the present crowded state of his studies, for the perusal of such a ponderous volume, but even supposing a student, to the neglect of something else, addressed himself to the task, the probability is he would become hopelessly bewildered. In almost every page, he meets with analyses of the writings of others, involving sometimes categorical statements of opinion, all of which, he would conclude, must have an equal claim on his attention; or, in another part, he finds that B did something on which he founded an opinion, which, however, is combated by C. This second party, C, repeated the experiments of A, made many years ago, and found results which are contrary to those of B, and consequently he supports the theory of A, with slight modifications. Or the student is wearied by finding the discussion of the function of an organ encumbered with references to the labours of observers in all parts of the world, some of whom have names which he cannot hope to pronounce. This mode of treating the subject is quite in keeping with the character of the work as a book of reference, but it makes it nearly useless to the average student. It must not be forgotten that a student has a definite amount of work to do in four years. He has the dread of examinations before him; these, under the present system, stare him in the face day and night; and he has to study at least ten subjects in addition to physiology. What he wants is a clear statement of the leading facts upon which the general principles of the science are based. With Carpenter's "Principles" in hand, he is bewildered amongst so many supposed statements of facts that he misses the general principles. He knows more of what men think they have done and discovered than of those great truths in physiology upon which the practice of medicine and surgery is founded. After a student has passed all examinations, then he should read Carpenter—but not till then.

The next work on our list is Dr Flint's Text-book (No. 2). It is a condensation of the author's elaborate treatise in five volumes. It has two merits we desire particularly to allude to. First, it contains excellent historical accounts of some of the principal discoveries in physiology, such as the circulation of the blood, the functions of the lymphatics, the glycogenic function of the liver, the functions of the various parts of the spinal cord, etc., etc.; and, second, it gives a more complete description of the physiological anatomy and functions of the cranial nerves than is to be met with in any other text-book of physiology. The introduction of historical matter, along with copies of engravings from the works of the older authors, is, we think, a most commendable feature. It is well to teach youth what the old fathers in physiology knew, and how they arrived at those discoveries of which we now reap the benefit. Histological details are illustrated by engravings of micro-photographs, which we must say are very poor. Micro-photography used in this way is not of great service. The attempt to obtain accuracy of detail is missed in the blurred appearance of the photograph; and there is not the same clearness and sharpness of delineation as there ought to be in a drawing of microscopical tissues suitable for teaching purposes. The section on sexual generation is fully written, and there are details regarding the preliminary steps of fecundation (such as the sexual orgasm) that we have met with nowhere else. Dr Flint's work, though large, is not encumbered by references to authorities, and consequently it is less confusing to a student than that of Dr Carpenter. Were it not for its great size, we would have no hesitation in recommending it as being the best book for the student of medicine; but regard for his interests compels us to withhold at present the recommendation, in the hope that Dr Flint may be induced to issue a more abridged work upon the same general plan. In the meantime we can strongly recommend it to the profession, more especially to those interested in diseases of the nervous system, who attempt to unravel the symptoms of a "nervous" case by the application of knowledge regarding the physiological connexions of nerves with the centres, and anastomoses among themselves.

Dr Dalton's book (No. 3) is a sixth edition of his well-known treatise. It has earned a well-deserved reputation in its day, both in America and in this country. Its special feature is the great prominence given to the reproductive functions, and to the skilful way in which these functions are described and illustrated. It is a work, however, which has the fault of not being quite up to the position of modern physiology. The account given of the circulation of the blood, of the relation of the circulatory to the nervous systems, and of the minute structure of tissues, is very meagre, viewed in the light of the work accomplished by physiologists during the past ten years. If the section on generation were published separately, with fuller details as to development of special parts, it would constitute the best work on generation and embryology for the general reader in the language.

We next come to Hermann's Elements (No. 4), translated from the German by Professor Arthur Gamgee. This work, after passing through five editions in German, is now placed before English readers. We have first of all to congratulate Dr Gamgee on the successful accomplishment of the task, which must have involved a great deal of literary labour. To say to such a man that the book has been ably translated is small praise. Dr Gamgee, we may feel assured, did not undertake the work without much consideration as to its ultimate value in the promotion of physiological knowledge in this country. He must have thought that a text-book was required in this country, and that Hermann's was likely to meet the want. We agree with him as to the first presumption, but not as to the second. No doubt a good text-book was wanted. Carpenter's was too large; Kirke's too vague and unscientific; Bennett's too one-sided and inaccurate. Another was required for the student of medicine. But for reasons which we regret to have to state, Hermann's is not the book for the average student at our schools.

There are two aspects in which to view such a work,—first, as it is in itself, and second, as a medium of the communication of knowledge to a certain class of students. With regard to the first of these views, there is much to be said in commendation of Hermann's Elements. The great reputation of the author as a physiologist who has contributed largely to the subject, both as a critic and original worker; the attempt made (first) to treat physiology as a branch of physical knowledge, and (second) to introduce into its nomenclature a free use of terms used in purely physical science; and the recognition of the fact that there is much we do not know in physiological processes, coupled with a resolution not to shuffle out of a difficulty under a cloud of words,—are considerations which place this work in the very forefront of physiological text-books. We consider, therefore, that physiologists are under a debt of gratitude to Dr Gamgee for having the boldness to lay such a book before our medical students; and the regret we have is that our average student is not in a position to understand it.

It must not be forgotten that the great majority of our students of medicine at present, when they arrive at the systematic study of physiology, are deficient in even the elements of two branches of knowledge which are essential before they can tackle such a work as the one we are now considering. First, they have had no training, not even, in many cases, a course of lectures, on physics. They no doubt "pass" on elementary mechanics at the preliminary examination, and a few may take natural philosophy in its higher branches as one of the optional subjects. To such men, Hermann's Physiology, in many of its chapters, will be nearly unintelligble. Second, the majority of students know little of organic chemistry—at all events, not enough to follow intelligently the first part of Hermann's Physiology. No doubt they know more of this department of knowledge, at least in Edinburgh, than of natural philosophy, but still they have not that familiarity with the general principles and facts of organic chemistry which can enable them to follow a lecturer into the difficult domain of the chemistry of animal products and processes. It appears to us, therefore, that with the present state of knowledge amongst our students of these subjects Hermann's book is premature in its appearance amongst us. It is, in point of fact, too high for our ordinary student. When the student is better educated in the subjects above specified, then he will be in a position to study such a book with profit. To prescribe this as a text-kook at present is simply to lay on the poor student a burden grievous to be borne. This is an unsatisfactory state of matters, and may occasion surprise and regret when we think that this work is a favourite text-book in Germany. But we imagine some one at once asking—"*Cui bono?* What is the advantage of being able to understand such a book?" The answer is obvious. The progress of research and thought in modern physiology has been (1) to bring so-called vital processes, obeying presumed laws of their own, into the domain of physical inquiry; (2) to apply to physiological phenomena the great law of the conservation of energy which is now the guiding principle in physics; and (3) to use, in descriptions of physiological processes, terms hitherto used only in physical research which have a definite meaning and value. Hermann's book is distinguished above all others by the recognition of these facts. It introduces a new nomenclature into physiological science—a nomenclature which will eventually lead to more accurate views in many physiological and pathological processes. In many parts misty and speculative, and in a few not very explicit or accurate in formation, it is still a great work, the reflection of a master mind, and we hope the time is not far distant when it, or some such work, will be the text-book of our medical students. To the physiologist, and to the practitioner who desires to keep abreast of the most advanced thought on physiological subjects, we strongly recommend it.

Professor Küss's Manual (No. 5), in addition to being an excellent general résumé of the subject, has certain peculiarities of its own. Unlike those books we have already alluded to, it devotes a considerable amount of attention to cellular physiology. It then passes on to the physiology of the nervous system and contractile structures, before discussing the nutritive and reproductive systems. Professor Küss attaches great importance to the functions of the epithelial globules in absorption and secretion. The reference to the electro-physiology of nerve and muscle is very meagre. There are a great many facts alluded to by Küss, more especially from French authors, which are not alluded to by other physiologists. On the whole, we consider this book a well-conceived text-book for the advanced student in physiology. The general arrangement of the work is different from most, as may be seen by the following headings of the various parts:—1st, General physiology—the globule or cell; 2d, The nervous system; 3d, Contractile elements; 4th, The blood and its circulation; 5th, A general view of epithelial globules and epithelial surfaces; 6th, Digestive system; 7th, Pulmonary mucous surface—respiration—heat—phonation; 8th, External integument; 9th, Organs of the special senses; 10th, Urogenital system—embryology. For the student it has the advantage of moderate size.

Mr Hilles's work (No. 6), now in its second edition, is a compendium of the subject. It has far too much, however, of the character of a book intended for "cramming" students for examinations. It will not bear comparison for one moment, as regards scientific exposition, with any of the books we have already noticed. With the exception of the second chapter, the work is a compilation. Here, however, Mr Hilles brings forward his own views on debatable points. He is evidently a strong upholder of the theory of "Life" as a special principle, of the old theory of Paley that there is an "adaptation" seen in organic nature, now so much ridiculed by some, and forgotten by others; and he evidently dreads Darwinism, or, properly, Lamarckism, as a taint to be avoided. The chief fault of his work is, that it is not precise. It does not contain a scientific account of the circulation of the blood, more especially in its relations to the nervous system. It is also too evident that frequently the author has obtained the information he professes to give second-hand, without consulting the original authorities. We regret we cannot recommend the work either for practitioners or students.

Next we arrive at Professor Beaunis's "Nouveaux Eléments" (No. 7), which is in many respects one of the best works of the series. Although voluminous, it is an excellent example of the exquisite power the French possess of scientific arrangement and exposition. After a short introduction, stating the general scope of the work, there is a preliminary chapter on the *Physiological Laboratory*, in which good general directions are given regarding the rooms, the mode of conducting vivisectional experiments, the apparatus required for microscopic and chemical research, the physical instruments and apparatus likely to be required, and, lastly, a very short account, with six plates, of the anatomy of that most valuable animal to which humanity is so much indebted—the frog. Then follows a chapter entitled "Prolégomènes," in which the author treats of force and movement; the general character of living bodies; the distinctive characters of animals and vegetables; and, lastly, the place of man in nature.

The second part treats of physiological chemistry, in which the author includes a description, not only of the principal proximate principles, but of the various liquids found in the human body, such as blood, lymph, chyle, urine, bile, etc.; and of the secretions, such as saliva, pancreatic juice, etc. We question the wisdom of separating the chemical description of these fluids from the general description of their production and action, as it does not give a complete view of the subject, but necessitates the perusal of separate portions of the work.

The third part relates to the physiology of the individual, while the fourth treats of the physiology of the race, embracing the discussion of such topics as the characters and the origin of species, as relating to man's position in the animal kingdom, the varieties of the human race, the various theories as to the origin of man; and the condition of prehistoric man. The special character of the work, however, is the section on the physiology of the senses and of voice. We have no hesitation in stating that no text-book of physiology, since the time of that of J. Müller, as translated by Baly, presents an account of the present state of knowledge regarding the physiology of the senses so complete as that of Beaunis. The sub-sections on the ear and eye are models of lucid exposition. They contain a clear account of the results of all the more recent researches of Helmholtz and others put in few words by one who evidently thoroughly understands these somewhat abstruse subjects. The mode of treating the subject of vision, for example, may be gathered from the following headings of paragraphs and sub-sections on the physiology of vision, which we quote as a proof of the exhaustive character of the work:—Of light; physical laws of the reflexion and refraction of light; construction of a refracted ray; construction of the image of a point; the dioptric system of the eye; the schematic eye; formation of the retinal image; diffusion images; emmetropia, myopia, and hypermetropia; aberration of sphericity in the eye; astigmatism; aberration of refrangibility in the eye; entoptic phenomena; accommodation; Listing's law; optometry; mechanism of accommodation; movements of the iris; innervation of iris; of excitants of the retina; of the proper light of the retina; of retinal excitability; the *punctum cæcum;* Volkmann's experiment; of the yellow spot; of the peripheric parts of the retina; of the mode and nature of retinal excitation; of the conditions of retinal excitation; duration of impressions; measure of intensity of luminous impressions; persistence of impressions; of consecutive monochromatic images; of sensations of simple and compound colours, according to the experimental methods of Helmholtz, Lambert, Czermack, Maxwell, etc.; of the characters of colour sensations; on the geometrical representation of colours; on the Young-Helmholtz theory of colour-perception; on consecutive coloured images; on the contrast of colours; movements of the eye; action of ocular muscles; monocular visual field; binocular vision; binocular diplopia; fusion of double images; convergence of visual lines; binocular vision of colours; visual perceptions; externality of visual sensations; localization of visual perceptions; continuity of visual perceptions; notions furnished by vision, such as the size of objects, the distance of objects, direction, solidity; movements of bodies; the general physiological properties of the eyeball, such as the ocular circulation, intra-ocular pressure, etc.; and, finally, the protective apparatus of the eye.

At the end of each section, Beaunis gives a bibliography of the subject, which seems to be carefully compiled, and cannot fail to be of service. On few points is the work deficient. We do not find a satisfactory account of recent researches on the innervation of the heart. Taking it all in all, it is an able work. Less speculative than Hermann, it is quite as scientific in its tone and scope, and, with the exception of the separation of the account of the chemistry of the secretions from an account of their production, we think its general plan superior. The book should be in the library especially of all who are interested in the physiology of the senses, and in the important psychological questions relating thereto.

The last work on our list is Dr Letourneau's short and clever exposition of biology (No. 8). It is a popular work, intended for general readers, and is well suited for advanced classes in public schools, and for any well-educated person who desires a scientific statement of the generally accepted views of biology, without professional details. It includes chapters on organized matter in general; on the primary phenomena of life; on growth; on generation; on movement; on innervation; and on the influence of physical forces in biological phenomena. There is no work precisely of the kind in the English tongue, and we think a translation would be a good service.

The Student's Guide to Dental Anatomy and Surgery. By HENRY SEWILL, M.R.C.S. London: J. and A. Churchill. 12mo.

THIS is one of those concise epitomes of pathology and practice, which the dental student will find very useful. The author in his preface states that he has attempted to epitomize the main facts elucidated by recent authorities without entering upon controverted topics, and in this attempt he has been most successful. A number of illustrations, principally from more elaborate works on dental surgery, figure in the volume before us, and add greatly to its value and perspicuity. The publishers deserve much credit for the manner in which the book is got up, and it seems only fair to them to mention that it is not the first or only manual of the kind they have produced. In the preface it is remarked that the "projectors of the 'Student's Guide' manuals have considered a work on dental anatomy and surgery a desirable addition to the series;" now, if the Messsrs Churchill be here alluded to, it was only in 1871 that they published "The Hand-book of Dental Anatomy and Surgery," a work very similar to that at present commanding our favourable notice.

A Text-Book of Electricity in Medicine and Surgery. By GEORGE VIVIAN POORE, M.D. London: Smith, Elder, and Co.

THIS is the best of the smaller works on the subject, and, indeed, is preferable to many books of greater pretension. The therapeutics of electricity are yet so imperfectly understood, the knowledge of it which we do possess is so essentially transitional, that no text-book can be expected to last for any length of time. Nevertheless, it is essential that the practitioner should have some means of ascertaining definitely what has been done, and of determining the powers and limitations of an agent which he will frequently be called on to use.

Perhaps the best part of Dr Poore's book is that which comes first. The general principles on which the use of electricity is founded are given with great clearness, but without that excess of detail which is likely to prove wearisome to those who do not wish to make a special study of the subject. The description of apparatus is also good. Dr Poore, for medical purposes, prefers the Leclanché elements. Foveaux's Smee is an extremely convenient form of battery, which of late years has come into very general use, and, indeed, it would be difficult to surpass it. The Leclanché battery, however, presents the advantage of greater durability.

Dr Poore points out the propriety of having, connected with whatever instrument be selected, a regulator, commutator, and rheostate, additions which are extremely convenient, but generally neglected.

Chapters three and four, on the physiological actions of electricity and the methods of using it, are also worthy of careful study.

The therapeutics are divided into three heads, viz., electricity as an aid to diagnosis; electricity as a stimulant; and electricity as an anodyne or sedative. Here, as might have been expected, for the reasons already mentioned, the author becomes less satisfactory. The chief fault of the book in this part is the tendency to profuse quotation. Doubtless, in this way, a great deal of valuable information is communicated; and as certainly it is extremely difficult, in treating of electricity, to enunciate clear and definite statements when so much is vague and uncertain. But it is well in a text-book to aim at a crisp and concise expression of opinion, rather than an exhaustive exposition of the theories of others. Those parts of the book which treat of topics which Dr Poore has evidently thought out and made his own, are infinitely more satisfactory to the reader than the pages of quotation.

These remarks apply very forcibly to the section which deals with the surgical uses of electricity,—a department, however, in which Dr Poore does not profess to have had much experience.

On the whole, however, the book is a well-conceived attempt to supply a want in medical literature, and will repay a careful perusal.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION LV.—MEETING VII.

Wednesday, 3d May.—Dr GILLESPIE, *President, in the Chair.*

I. *Dr James Carmichael* showed a SPLEEN IN WHICH WERE NUMEROUS MILIARY TUBERCULAR DEPOSITS. It was taken from a boy seven years of age, who died of meningitis. There were caseous deposits in the lungs, as well as small granulations on the pleura. The brain presented the usual appearances found in tubercular meningitis, with the exception that there were no small granulations on the pia mater, the whole of which, however, was intensely congested. The central space between the optic commissure in front and the pons varolii behind was completely blocked up with sero-fibrinous lymph. The corpus callosum and fornix were softened. The peritoneum and abdominal organs were healthy.

II. *Dr George W. Balfour* exhibited—(1.) AN ANEURISM OF THE AORTA coming off from the ascending aorta, and rendering the valves incompetent. This aneurism consisted, first, of a sac the size of a large orange, which compressed the left bronchus, and to some extent the lung; and, secondly, of a smaller sac with somewhat thinner walls, which projected from the anterior part of the large sac, was the size of a walnut, and was adherent anteriorly to the thoracic wall in the third interspace. This patient had been several times under Dr Balfour's care, and had received great benefit from the use of iodide of potassium in full doses; and he had brought the preparation before the Society because it confirmed in a very striking manner the views he entertained as to the mode of action of that drug.

The relief during life had consisted in a diminution and almost complete disappearance of the pulsation in the third interspace, and along with that, a simultaneous remission of all the pressure symptoms—in particular, of those affecting the lung. This patient died under the care of another physician, after a very short illness of a catarrhal nature: the immediate cause of death having been œdema of the lungs. On examination after death, not a trace of a clot was to be found in the aneurism. Considering its situation, this was probably not to be wondered at; but, taken in connection with the very great and marked relief obtained from the use of the iodide of potassium, it afforded a pretty strong proof that clot-formation was certainly not the efficient agent in producing that relief. The symptoms in this case, as well as the physical signs, quite conclusively showed that there was a general diminution in the size of the sac; and in another patient treated during last session, in whom the aneurisms were external and could be felt, their contraction was very evident both to touch and sight. Upon making sections through various parts of the sac, and examining them microscopically, the intima was universally found to be diseased, granular, and degenerated, and irregular in its thickness, as if being worn away; the muscular layer of the media was tolerably dense, apparently hypertrophied, over the large sac close to the artery; over the smaller sac, at its external portion, it was still present, but granular, undergoing fatty degeneration. The elastic layer of the media was not atrophied or degenerated in any part examined, and the adventitia was considerably hypertrophied, particularly at the outermost portion of the smaller sac. This case seemed, therefore, to confirm his (Dr Balfour's) view, that the action of iodide of potassium was first of all to diminish the blood-pressure, by acting as a sedative to the heart's action—an action which, to some extent, might be due to the potassium (what Quincke had called "Kali Wirkung"), but which was not exerted to anything like the same degree by any other salt of potass with which he was acquainted; while it was also possessed to some extent, at least, by the analogous salt of sodium, a salt which, however, he had not much used, as it was in no respect preferable to the iodide of potassium, and, in particular, it seemed even more frequently, in his experience, to be followed by eruptions of hydroa. The second action exerted seemed to be a hypertrophying of the adventitia, and possibly to some extent, in some cases at least, of the media. The iodide of potassium was generally believed to act on the fibrous tissues; it had not yet been shown that it tended to hypertrophy them, and this action might be only apparent. For instance, the diminution of the blood-pressure by a drug which in no other respect interfered with the nutrition of the body, and whose use was not incompatible with the proper feeding of the patient, might possibly be regarded as only favouring the normal reaction of the hollow muscle and its fibrous sheaths against a disturbing force with which, alone and single-handed, it was unable to cope. This was a matter which as yet it was impossible to decide one way or the other.[1]

(2.) Dr Balfour showed the HEART, LUNGS, KIDNEY, etc., of a patient, the victim of innumerable attacks of EMBOLISM, and exhibiting the various results of that affection in every stage and in every organ of the body. The patient had been at various times under his (Dr Balfour's) care during the last four years, labouring under a dilated heart, from every symptom of which she had been twice over completely relieved.[2] Three years ago she had been seized with embolic hemiplegia during the visit, but every trace of that had completely disappeared in a few weeks. No other embolic phenomena had ever been observed during her residence in hospital till after her last admission, when she was found to be much wasted, very anæmic, and her heart as much dilated as ever. On putting her, as usual, under the influence of digitalis, the result was a perfect shower of pulmonary emboli, which were always accompanied by a rigor, sometimes by slight and temporary unconsciousness, and by a rise of temperature. Those symptoms were followed in one or two days by hæmoptysis, which lasted for several days, while a limited dull patch could usually, but not always, be detected, over which dry crackling crepitation was audible; the limited embolic phenomena were in one instance followed by pleuritic effusion, part of which was subsequently reabsorbed, while part remained present after death. During her last illness giddiness was suddenly developed, the peculiar phenomena of which could not be accurately determined on account of her great weakness; but after death a very probable cause for this was found in the shape of embolic softening of part of the cerebellum. A few days previous to death an embolus plugged up the left brachial artery, producing paralysis, pain, and great œdema of the forearm. After death the source of all these emboli, and of many others not related, was found to be thrombosis of the cardiac cavities, which had been before death pointed out as their most probable source; this opinion being based upon the entire absence of any thrombosis or dilatation of the veins of any kind, coupled with the general anæmic condition of the body, and the long-continued dilatation of the heart. The appendix of the right auricle was completely filled by a dense and partially organized thrombus, while in the left ventricle there were two or three somewhat dense thrombi the size of peas, one the size of a filbert, and another the size of a walnut; the centre of the upper part of the latter was in a state of fatty softening, while in its lower part there were two distinct cysts developed, one somewhat larger than the other. This complicated condition completely prevented all attempts at again rehabilitating the heart. With the view of dissolving any loose particles of the thrombi, carbonate of ammonia was given somewhat freely, and after a time the digitalis again commenced in small doses; but no sooner did the pulse and cardiac force exhibit any apparent increase in strength, than some fresh embolus at once occurred; and at last all attempts at a cure were given up, and nothing but food and stimulants administered. The patient died, exhausted from the effects of her repeated attacks of embolism, the asthenia being aggravated by the weakness of the cardiac muscle, and the incompetence of the cardiac valves. After death, besides the thrombi referred to, all the cavities of the heart were found to be considerably dilated, and slightly hypertrophied. Eight fingers could be passed through the right auriculo-ventricular opening, and four through the left one (cone measurement, 1·55 inch). But the most remarkable thing about this heart was that at times, and notably for several days before death, *its two sides pulsated separately.* On palpating the heart, it was found to beat 80 times a minute, while the radial pulse was only 40; and on inspecting the neck, the carotid artery was seen to beat synchronously with the radial artery and with one of the cardiac pulsations, while the jugular vein was both seen and felt to beat alternately with the carotid artery, and synchronously with the other beat of the heart. So far as he (Dr Balfour) knew, no similar case had ever been recorded, and he himself had only seen one other like it. This also occurred during last winter session, and the phenomena in both cases were precisely similar. Nay, the very histories in both cases were very much the same. Both were females, and suffered from dilated heart, without valvular disease, but with persistent jugular pulsation, and also from repeated attacks of embolism. Both had also suffered at a former period of life from some inflammatory attack of the neck. Both of these females, during those attacks of peculiar cardiac pulsation, were extremely hyperæsthetic, especially over the upper part of the trunk; and in both, when this peculiar pulsation was absent, it could almost invariably be reproduced by pressure on the pneumo-gastric nerve, which gave rise to considerable pain. For the present, Dr Balfour contented himself with merely cursorily recording these cases, which he believed to be perfectly unique. Sphygmographic tracings both of the carotid and jugular pulses had been obtained, and a full account of both cases would be presently published by his late clinical assistant, Mr Roy, as soon as the nervous relations of the parts had been carefully examined.

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[1] This case is that of the woman Murray, cursorily referred to at p. 323 of Dr Balfour's work on Diseases of the Heart and Aorta.

[2] The subject of this case is Euphemia Lawson, whose previous history will be found at p. 173, *op. cit.*

III. *Dr J. B. Tuke* exhibited A BRAIN PREPARED BY CHARCOT'S PROCESS. The brain to be preserved is steeped for six weeks in diluted nitric acid, and then allowed to dry. By this means it shrinks to about one-fourth of the natural size, but preserves most perfectly the anatomical relations. Professor Charcot uses such preparations as a means for recording lesions of the convolutions, instead of employing flat charts. Brain so prepared must also prove useful for teaching purposes.

IV. *Mr Annandale* then read a NOTE OF A LARGE REDUCIBLE INGUINAL AND FEMORAL HERNIA AFFECTING THE SAME SIDE, SUCCESSFULLY TREATED BY OPERATION, which appears at page 1087 of this Journal.

The *President* said the Society was indebted to Mr Annandale for his most interesting case, as well as for his bringing the patient for the inspection of the members. For his own part, however, he would like to see him again in about nine months, because his experience of radical operation had been that the hernia recurred after a time. In Mr Annandale's case there seemed to be a fair prospect of success as to the inguinal hernia; but there was a slight recurrence of the femoral one. The latter however, not being so large as formerly, might be restrained by a truss. He noticed, too, that on the other side the patient seemed likely to have another hernia, so that he might again require a similar operation for it.

Mr Chiene assisted Mr Annandale at the operation, and had felt pleasure at the prospect. This pleasure arose from the fact of Mr Annandale's intention of ligaturing the necks of both the sacs, thus performing such an operation for the cure of a double hernia on the same side for the first time. The methods of operating for the radical cure of hernia were chiefly three in number. Wutzer plugged the canal; Wood stitched the aperture; while others ligatured the neck of the sac with silk or silver wire. Now Wutzer and Wood's plans, as their President had told them, generally failed in the long-run. In Mr Annandale's case he believed the inguinal hernia would not recur. From 1867 he had been watching for cases of complete or partial closure of the neck of the sac. He now showed two such, where the opening was partially closed. In the one it admitted the little finger, and a cure was going on owing to the pressure of the truss; while in the other, only the handle of a pen could be introduced. These two cases show the process of cure going on by thickening and contraction of the neck of the sac. Now, at the time of the operation, he felt that the weak point was in Mr Annandale's plugging the crural canal by means of the inguinal sac; and the present state of the patient corroborated this opinion. He regretted that Mr Annandale had not been able to ligature the necks of all the sacs. Had this been done, he felt sure that the result would have been as good in the case of the femoral as of the inguinal hernia. He therefore believed that the true way to operate in such cases was that based on the pathological results of cases such as he had shown, viz., that sac contents should be returned, and the neck of the sac ligatured; and that they should not attempt to cure by plugging the canal, or by stitching the walls of the canal.

The *President* asked whether the two cases alluded to by Mr Chiene had been seen in the pathological theatre, or during life.

Professor Simpson remarked on the absence of any distinct history in Mr Chiene's cases.

Mr Bell felt loath to cast any doubt on Mr Chiene's pathological facts, and the method for radical operation he deduced from them. It would certainly be a happy consummation if a radical cure of hernia could be accomplished; but, along with the President and Professor Simpson, he would have liked to have seen Mr Chiene's cases during life. Professor Roser had found many hernial sacs after death without any contents; and his theory therefore was, that the sac was first formed, and, as it were, waited for the bowel to enter. This it might never do. He would therefore explain cases where the sacs were empty as possibly being cases of hernia at that stage where the bowel had not yet descended. In the absence of history in Mr Chiene's cases, they had no evidence that they were not of this class. At present, he was watching the case of a little boy, a seven-months' child, where there was a true umbilical hernia, and an inguinal on each side, one of these being probably double. By proper management with trusses, there was now no descent of abdominal contents; but still he was not prepared to call it a case of radical cure, but he believed that the hernial sacs are still present in it. He had recently, in all his hernial operations, stitched up the necks of the sacs. Before sitting down, he would like to refer to a case in which he had recently operated for strangulation. The patient was a woman, sent in by his friend Dr Affleck, where the bowel had been down for six days. She was very low, with stercoraceous vomiting and a cadaveric odour of breath, but he operated and found the bowel dark brown, though still lustrous. He returned it, but kept the sac out. The result was, that the "sac" died; and now, four weeks after the operation, there was no tendency to protrusion, and a pretty deep sulcus remained, with much matting of the tissues. He thought Mr Annandale's case interesting, and very creditable to him.

Mr Chiene explained that in both his cases he had been asked to go to the post-mortem theatre to examine hernia cases; one of them, indeed, was known to have worn a truss. The reason of his being asked was, that he had requested such cases to be brought under his notice.

Dr A. G. Miller had not heard the whole of Mr Annandale's paper, but he wished to throw light on the present discussion as to the natural closing of hernial sacs. He had seen one case of spontaneous radical cure of hernia in the case of a man discharged from the army for a double inguinal hernia. He was requested, in this case, to operate for strangulation. It turned out, however, to be acute hydrocele of the hernial sac. This he tapped; and since that time there had been no return of the hernia. Closure of the neck of the sac was evidently nature's method of curing a hernia, and he thought that ligature of the neck by the surgeon imitated this.

The *President* pointed out that Dr Miller's case might have been only acute hydrocele of the cord. Many of the members must have seen supposed cases of strangulated hernia turn out to be this.

Dr Cadell and *Professor Simpson* pointed out that, in children, cure of cases of reducible hernia was evidently brought about by closure of the abdominal ring, not of the neck of the sac.

Mr Annandale thanked the Society for the kind reception given to his paper. Dr Miller's case, of which he had seen a similar instance, he believed to have been hydrocele of a hernial sac. While he held that there might be cure of hernia in young patients by closure of the opening, as Mr Bell and Professor Simpson had pointed out, still he thought that in other cases, it was due to contraction of the neck of the sac. He had repeatedly seen cases of large sacs with partially obliterated necks, and it was this fact that led him to propose the operation he had just described.

V. *Dr Angus Macdonald* then read a paper by *Dr Turner* of Keith on PASSIVE CEREBRAL CONGESTION, which will appear in a future number.

The *President* and *Dr G. W. Balfour* remarked on the interest of the paper, and of one of the cases illustrating it.

OBSTETRICAL SOCIETY OF EDINBURGH.

SESSION XXXV.—MEETING VIII.

Wednesday, 22d March.—Professor SIMPSON, *President, in the Chair.*

I. *Professor Simpson* exhibited TWO DRAWINGS, which had been kindly sent by Mr Harrinson of Reading, illustrative respectively of HÆMORRHAGE DURING PREGNANCY, AND POST-PARTUM HÆMORRHAGE. The first sketch had been shown to the obstetrical section of the British Medical Association in August, when Mr Harrinson gave the following history of the case:—"Mrs M., aged 36, was two months advanced in her ninth pregnancy, at the end of March 1875, when she received a great shock from a relative dying suddenly in the house. Hæmorrhage followed. It returned in April and in May. From the beginning of June to the time of her delivery, the hæmorrhage was off and on. It was worse at night; and, on rising up, a rush of nearly colourless fluid took place; was never safe from that. She went on in this way, keeping her room, till 4th July, when some fibrinous masses, the size and thickness of a florin (decolorized clots) escaped, with relief to pain, from which she had been suffering for some hours. Dark discharge alternated with watery, without clots, till 7th July, when a dead child, of five months, was born There was no hæmorrhage either before, during, or after the labour. The placenta was pale and soft. The membranes, for the space of about two inches, were separated from the fœtal surface of the placenta, and the rent was filled with some small clots of blood and fibrinous shreds."

The following were the notes of the sequel to this case, which Mr Harrinson had just sent, along with the second of the sketches:—

"Convalescence was not satisfactory. The patient had weight, bearing down, and discharge, frequently coloured. *Examination, 14th August.*—Uterus low down, enlarged, inclining too much backwards. Cervix swollen, and parts altogether fuller and more tender than usual; five weeks after confinement, subinvoluted. *26th August.*—A substance, the size of a walnut, was expelled, with considerable pain and hæmorrhage. It was covered with fine fibrinous shreds and minute clots of blood. On washing it, a strong capsule was seen, with here and there on it a number of small, white (fatty?) masses. On cutting it nearly through and reflecting the halves, a whitish nucleus was seen; then a layer of blood, in coarse meshes, and then the capsule, as before described. The nucleus appears to be a portion of placenta, enveloped by a clot of blood, which on the surface has become organized and slightly attached to the uterus. The presence of such a body in the uterine cavity would be, I think it will be admitted, quite sufficient to account for the unsatisfactory result of the examination on 14th August. Practically it will be useful to bear in mind that the expulsion of a semi-organized clot, with a bit of placenta for a nucleus, and keeping up during its retention all sorts of inconveniences, is an event that *may* take place some weeks after a confinement. I have not seen a similar case. Since this occurrence the convalescence of the patient has been uninterrupted."

He (Dr Simpson) remarked upon the interest of this case as bearing upon the occurrence of hæmorrhage both during pregnancy and after labour. With regard to the continuance of discharge and other symptoms of distress, from which the patient suffered after the labour more especially, he thought the case an excellent illustration of the necessity of securing the complete evacuation of the uterus, on which he had insisted in his communication to the Society at their last meeting. So long as there was a fragment of the placenta within the uterus it kept up the symptoms described by Mr Harrinson; from the time that its complete evacuation was effected, convalesence progressed satisfactorily.

II. *Professor Simpson* read notes of an interesting case of CHOREA GRAVIDARUM, and at the same time showed a preparation of the UTERUS AND MEMBRANES OF THE BRAIN. The patient was an unmarried woman, twenty years of age, who had died in the fifth month of utero-gestation, after having suffered for about two months from bilateral chorea of a very intense type. There had been in this patient a combination of the conditions that have severally been noted as associated with the development of chorea. For, 1st, She had been the subject of an attack of rheumatic fever about eight years previously, and there had been a recurrence of it, in a milder form, shortly before the chorea appeared; 2d, She had been the subject of a recent attack of scarlatina; and, 3d, She was pregnant.

Dr Underhill thanked Professor Simpson for his paper, and remarked that the case was both rare and interesting.

Professor Simpson had brought the case under the notice of the Society because it was important to get as complete records as possible of a form of disease so comparatively rare. In the latest notice which he had seen in Schmidt's Jahrbücher, the total number of cases collected from all sources amounted only to some seventy-three. With the Society's permission, he would send the extended notes of the case for publication to the Obstetrical Journal.

III. *Dr Smith* read the following NOTES OF A CASE OF TRIPLETS. On Friday, 10th March, I was called to see Mrs Anderson, 20 Lauriston Street. On arrival, I found she had been safely delivered of triplets. There was a midwife in attendance. Mrs Anderson is a short thin woman, 36 years of age. She began to menstruate when 15 years old. She was married in 1862. Previous to this occasion, she had been seven times pregnant; all the births were single. On 8th April last, she was delivered of a still-born male child at the full term. Its death she attributed to a fall shortly before its birth. On 15th May, she menstruated for the last time, when she noticed that the discharge, instead of being scanty as usual, was excessive. The rapid increase of the abdominal tumour was early noticed. On Tuesday, 26th October, when exhausted, she fainted, and when recovering felt for the first time fœtal movements. The cracks in the skin of the abdomen were well marked and numerous—a broad one extending from side to side being specially noticed. During the last weeks there was a median depression dividing the abdomen into two lateral enlargements. Up to the commencement of labour there had been no hæmorrhage externally, and at no time was there any œdema of the legs. On 5th March, she began to have pains regularly. In the evening there was a discharge of some blood, which continued till after delivery. After this the fœtal movements ceased to be felt in the left side, and were confined to the lower part of the right side. The pains continued at intervals. On the 9th, about 9 P.M., there was a discharge of liquor amnii, and again at 9 next morning a second flow. Of the children the first survives, the second was decomposed, the third seemed quite recently dead. The breech of the surviving child presented first. It was born at 1.45 P.M., on 10th March. The decomposed child presented next with its head, and was expelled at 2.5, the membranes being entire. The third child presented its head also, and was born at 2.20, the membranes in this case likewise being unruptured. Its birth was followed immediately by the expulsion of the double placenta of the two last born. This in turn was followed by the single placenta of the first child—one pain expelling the third child and the placentæ. All the children were males. The third child was fully developed. Its abdomen was swollen and hard. The second child appeared to have been dead some time (probably about two weeks). Its scalp was flaccid, and seemed to contain little else than the cranial bones. The first, which survives, is a healthy child. At birth there was an ecchymosed patch over the left buttock. A few days after delivery there was some œdema over the pubes and in the left leg, probably owing to irritation of the umbilicus. On examining the secundines, the fœtus first to present was found to have had a separate placenta, which was small, being about five inches in diameter. Each of the other fœtuses had a separate chorion and amnion, which were attached to a common placenta. There was no trace of a division on the maternal surface of the placenta. There were the remains of several extravasations in the substance of the part of the placenta to which the decomposed fœtus was attached. Her mother's sister had once twins, but that is the only known instance of a plural birth among all the relations of her family.

Dr Dickson remarked that he had lately had a case of triplets in a fourth confinement. Two of the children lived.

Professor Simpson mentioned that within the last two months in Edinburgh three cases of triplets had occurred.

After a few remarks from *Dr Cappie*,

IV. *Dr Underhill* read a paper on the STRUCTURE OF A TRUE MUCOUS POLYPUS, and illustrated his observations by microscopical preparations. (This paper appeared in the Journal for May, p. 995.)

Professor Simpson thought the Society indebted to Dr Underhill for bringing before them such a careful anatomical description of these mucous polypi. They were very common. With regard to their connexion with other forms of disease, he had frequently observed them existing as a complication in cases of fibrous tumour. In these cases they often gave rise to serious bleeding, which was at once stopped by their removal. In the same way they were found to exist as a complication in cases of malignant disease. He had lately had two cases in elderly females in which there was great flooding arising from the presence of mucous polypi. In the first case he found the polypus pointing through the os externum, and removed it by the ecraseur; in the second case, that of a widow, the lady, although 70 years of age, still thought she was menstruating. In both these cases the hæmorrhage had been irregular, and in the first the bleeding was very profuse. She had been quite free from hæmorrhage, however, since the removal of the polypus, which grew, as most of them do, from the interior of the cervix, and was of the size of a large gooseberry.

Dr Underhill, in reply, stated that he had found great differences in the structure of these polypi, and many further observations were yet required to elucidate the pathological peculiarities of the different varieties.

Part Fourth.

PERISCOPE.

COCA.—The following is a curtailed notice of a review of a work on this subject in the *Revista Médica de Chile* for November last:—"In the last number of this review, we mentioned that the Medical Society had been presented with a beautiful work on this interesting plant by Dr Manuel A. Espinosa. To-day, having read it attentively, we wish to give a succinct account of it. After a very curious history of the plant in question, which abounds in most interesting data, which reveals in its author great erudition, and a spirit of extensive and conscientious investigation, after a complete and perfect botanical description, and after enumerating the localities in America where it is found or where it is cultivated, he enters into the question of its qualities, which is undoubtedly the part on which he is best qualified to speak, and which is also the most worthy of notice. He cites various analyses of coca leaves, which are made up of, according to Miemann, of cocaine, of a particular kind of wax, tannic acid, solid essential oil, chlorophyll, and woody matter. He does the same with the *llipta* or alkaline matter, which the Indians who use it mix with the coca leaves. This is made into a bolus, which they call *acullico*, and which must be chewed in order to extract from the leaves their active principle. He attributes to the cocaine the effects which the chewed leaf produces on the body. Cocaine is a nitrogenous alkaloid, able to neutralize acids, and which forms with them crystallizable salts, especially with acetic and hydrochloric acids, and whose atomic formula is $C_{16}H_{20}NO_4$, and an equivalent of water. The action which *llipta* produces on the coca is thus perfectly explained; the cocaine existing in a state of combination with organic acids, any alkali or alkaline carbonate will loosen it from its combination, thus favouring its solution in the saliva, and its ultimate absorption. As cocaine is very soluble in alcohol, and especially so in ether, the best pharmaceutic preparation of coca will be the etherial alcoholic tincture. With the object of making observations worthy of credit, he made experiments on his own person and on a medical student, who willingly lent himself for experimentation. Wishing specially to discover its influence on tissue waste, at the same time they made use of an identical alimentation, and each day carefully observed the quantity and quality of the excretions, specially the pulmonary and the urinary. During the twenty-six days the observations lasted, they noted the temperature of the body and the state of the chief organs. They did not notice the absolute loss of appetite which is attributed to the use of coca, still less the hallucinations, full of poetry, which Dr Montegazza has described with as much exaggeration as enthusiasm. Thus, the *coquero*, believing himself transported to a thousand and more diverse worlds, mounted on two leaves of coca, exclaims: "*God is unjust for having made man* *able to live without coca. I would prefer a life of ten years with coca to one of ten thousand centuries without it.*" Neither did they feel that irresistible desire to skip about, which other observers say they have noted, which incited them (according to their own relation) to jump over the heads of persons round about them. The only thing they could observe with exactitude, was a slight headache, a sensation of heat in all the body, and sleeplessness. Their strength and energies seemed to be augmented. The daily examination of the urine, and of the product of the pulmonary exhalations, was an objective point of the experimentation. The quantity of urea in the urine, and of the carbonic acid in the expired air, underwent a notable increase, both phenomena being preceded by acceleration of the pulse and the respiration, and by a slight augmentation of temperature. Dr Espinosa looks upon coca as a special stimulant of the nervous system, whose gentle and continuous action is quite characteristic."

Part Fifth.

MEDICAL NEWS.

OBITUARY.

Lewis Hay Thatcher, M.D.—Since our last, another esteemed Edinburgh practitioner has passed away. Dr Thatcher was the son of an eminent obstetrician whom many must remember with grateful respect, although twenty-three years have elapsed since his familiar portly figure was missed from our northern metropolis. But he was not the first of his family who belonged to the medical profession, for *his* father also stood deservedly high as a physician in the town of Dover, for many years towards the close of last century.

Dr Thatcher, although manifestly in declining health for a considerable time, continued to attend to his professional occupations with his wonted spirit and assiduity, until three weeks of his death, when a pulmonary attack, probably induced by exposure, and rendered more serious by pre-existing disease of the heart, terminated his useful life: the end being sudden. He had completed his fifty-ninth year in March.

Dr Thatcher, although a skilful and dexterous obstetrician like his father, did not confine himself exclusively to that department, but came to be regarded as a well-informed and sagacious physician, who took a friendly interest in his patients, and spared himself neither trouble nor fatigue in promoting their welfare. He was honourable and considerate in all matters of professional etiquette. He had a genial humour—always a good sign of a man; told a story well, almost dramatically; and had a decided taste for the fine arts.

We happen to know that his second son, Mr Charles Thatcher, a young man of great promise, is far advanced in his professional studies, and is almost certain to maintain if not enhance the reputation of the family name.

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1st Dec. 1875.—Paper by Mr Joseph Bell on operations for relief of extensive destruction of the vesico-vaginal wall, with discussion by Dr G. Stewart, Prof. Simpson, and Dr John Duncan; case of torpor of the liver and bowels from birth by Dr Cuthbert, 644–647.

5th January 1876.—Valedictory address by Dr Haldane; exhibition of pathological specimens, etc., by Messrs Annandale and Chiene, and Drs A. G. Miller and T. G. Balfour; paper by Dr Argyll Robertson on trephining the sclerotic in glaucoma, with discussion by the President, and Messrs Annandale and Joseph Bell, 739–749.

2d Feb. 1876.—Exhibition of pathological specimens, etc., by Drs Foulis and Craig; paper by Dr Matthews Duncan on the alleged occasional epidemic prevalence of puerperal pyæmia or fever and erysipelas, with discussion by Drs Gillespie, Ballantyne, Taylor, Bruce, Macdonald, Simpson, Chiene, Mitchell, and Temple Wright, 839–846.

1st March 1876.—Dr Cadell's paper on a case of lymphatic inflammation simulating venereal disease; Mr Joseph Bell's paper on paraffin epithelioma of the scrotum, with discussion by Drs Robertson, M. Duncan, Bruce, T. A. G. Balfour, and Messrs Chiene and Bell; Dr Littlejohn's paper on compulsory intimation of disease, with discussion by Drs M. Duncan, Andw. Wood, Wilson, Ballantyne, Bruce, and Mr Chiene, 938–944.

5th April 1876.—Exhibition of patient by Dr John Duncan, and specimen by Dr Finlay; paper by Dr Bruce on puerperal fever, with discussion by Drs M. Duncan, Carmichael, and Gillespie; paper by Dr Church on a case of pneumothorax, with discussion by Drs G. Stewart and Ballantyne, 1038–1039.

3d May 1876.—Exhibition of specimens by Drs Carmichael, G. W. Balfour, and Tuke; note by Mr Annandale on an operation for reducible inguinal and femoral hernia, with discussion by the President, Messrs Chiene and Bell, Drs Cadell and Miller, and Professor Simpson; paper by Dr Turner of Keith on passive cerebral congestion, 1114–1147.

Reports of Societies: Obstetrical Society of Edinburgh.—*14th April* 1875.—Exhibition of specimens by Drs Simpson and Duncan; paper by Dr Macdonald on a case of puerperal tetanus, with discussion by Drs Stephenson, Simpson, and Duncan; notes on intrauterine craniometry by Dr Duncan, 78–79.

12th May 1875.—Paper by Dr Foulis on the development of cysts in the stroma of the ovary, with discussion by Drs Macdonald, Simpson, Keiller, and Matthews Duncan; Dr Underhill's note of a case in which the fœtal heart was heard at an early period of pregnancy, 265–270.

23d June 1875.—Exhibition of specimens by Drs Underhill and Matthews Duncan; note by Dr Underhill on the uterine mucous membrane of a woman who died immediately after menstruation, with remarks by Dr Duncan; paper by Dr M'Rae on a case of twin conception, with discussion by Drs Simpson and Underhill, 343–345.

14th July 1875.—Exhibition of specimens by Drs Wilson and Simpson; note on a case of hypogastric fistula by Dr Rattray; exhibition of specula by Dr M. Duncan; paper by Dr James Cumming on uterine souffle and fœtal heart, with discussion by Drs Gordon and M. Duncan; paper by Dr Milne on obstetric therapeutics, with discussion, 549–551.

17th November 1875.—Professor Simpson's paper on sarcoma uteri, with discussion by Drs Keiller, Gordon, Duncan, and the author, 647–648.

24th November 1875.—Specimens exhibited by Drs Underhill, Ritchie, Jamieson, Rattray, and Bruce; paper on laceration of the perineum by Dr James Young, and discussion by Drs Carmichael, Peel Ritchie, Macdonald, Pridie, Ballantyne, Bruce, Burn, Rattray, Menzies, Milne, and Simpson, 648–651.

8*th December* 1875.—Professor Simpson's address on menstruation, 749–750.

12*th Jan.* 1876.—Exhibition of specimens by Drs Matthews Duncan and Bruce; paper by Dr Matthews Duncan against the pendulum movement in working the midwifery forceps, with discussion by Drs Bruce, Keiller, Macdonald, Young, Wilson, Gordon, and Simpson; paper by Mr C. Naylor on the influence of maternal digestion on fœtal pulse, with discussion by Drs M. Duncan, Macdonald, Bruce, Keiller, and Ritchie; paper on Hamamelis virginica by Dr Hewan, and discussion, 846–849.

26*th Jan.* 1876.—Specimens of calculi by Dr K. Duncanson; paper by Dr Duncan on inevitable lacerations of vagina, with discussion by Drs Underhill, Macrae, Gordon, Wilson, and Simpson, 849–850.

9*th Feb.* 1876.—Dr Matthews Duncan's paper on rupture of the perineum, with discussion by Drs Simpson, Macdonald, and Milne; Dr Underhill's paper on case of absence of uterus, with discussion by Drs M. Duncan, Milne, Macdonald, and Simpson, 944–945.

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